

AGAINST THE TIDE

A Critical Review by Scientists of How Physics and Astronomy Get Done

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Foreword
by Martín López Corredoira
and Carlos Castro Perelman

It is always necessary to take a critical look at the way in which scientific research actually gets done. While Philosophy and Sociology have long established themselves as full-blown academic disciplines with an ever-increasing literature, there is a dearth of such literature written by practising scientists. The aim of this book is to gather the views of some working physicists and astronomers on the influence of the social structures of science within which scientists are obliged to carry out their research, and examine the ways in which they are sometimes used in negative ways to destroy careers and hinder innovative research. At the present time there are no widely known academic outlets where scientists can express their opinions about the scientific establishment. Not so long ago there were astronomical journals where one could raise these issues but these journals have either ceased to exist or have been revamped into pure research journals. Physicists have no outlet for expressing their views—especially unorthodox views—on the nature of the scientific method and/or social structures affecting their research because journals for physicists are solely dedicated to research. This book aims to fill this current gap in the literature with a sample of critical papers.

The essay “Challenging dominant physics paradigms” by Campanario and Martin is a general analysis of the difficulties found by well-qualified scientists to challenge scientific orthodoxy. Particular cases of dissidence are reflected in the autobiographical odysseys narrated by Kundt, Arp or Castro Perelman. Castro Perelman tells us about the illicit, shameful censorship and blacklisting of scientists taking place in the electronic e-archives web-site <<http://arXiv.org>> and which is the most important internet site for preprints in Physics, Astronomy and Mathematics. Scientific and political elites in Western democracies control the system, according to Arp. Anonymity in the peer-review system is susceptible of corruption—says Marvin Herndon—and interferes with the objective examination of extraordinary ideas on their merits—says Van Flandern. These problems of science are worldwide and present in rich countries like the United States, as pointed out by Marvin Herndon, as well as in developing countries. Apostol talks about the corruption, decadence and mafias in Romania hidden behind the use of politically correct terms: “technological transfer”, “international cooperation”, “scientometrics”, etc. Like the example of Romania, many other countries have similar problems. The same problems in Physics and Astronomy are widespread in all fields of science and in all areas of research performed by humans. One representative of the text outside the fields of Physics and Astronomy is the essay by Bauer, where a critical study of analytical chemistry and the conflict of interests in science is presented. We considered it interesting because the context of his essay is applicable to all the sciences in general. The situation in Astrophysics is widely described in López Corredoira’s essay on the oldest profession. Kundt tackles some aspects of astrophysics too, Arp focuses in the research of cosmology, and Van Flandern’s article focuses on the solar system.

Whether the alternative ideas in these fields are correct is something at least doubtful because they are risky and speculative proposals. The vision of complexity in knowledge versus the mechanistic interpretation of Nature, as explained by Kirilyuk, is also challenging and highly philosophical. In any case, suppression of these ideas does not look the best way to do and filter science or promote progress in human knowledge. The possibility of removing good ideas from the stage is something harmful for the search of truth. Moreover, the Gold effect, mentioned by Kundt, by which a mere unqualified belief can occasionally be converted into a generally accepted scientific theory through the screening action of refereed literature, of meetings planned by scientific organizing committees, and through the distribution of funds controlled by “club opinions”, is another disturbing element for the search of truth. It leads to unitary paradigms in the sense expressed by Kirilyuk, unitary thinking not necessarily associated to the unique truth.

There are two main attitudes one could take towards the present scenario in science. On the one hand, in López-Corredoira’s chapter “What is research?”, or in Apostol’s chapter “Where is the Science?”, one finds a pessimistic view without offering any solution, in which the state of science nowadays is decaying and waiting for its death. Science had an important role in the history of Western societies, but it is now too eroded to serve the ideal of a “science for the sake of science”. On the other hand, the conclusions of many authors along the book guided by the same *leitmotiv* of “freedom of research”, envision some hope to improve the current system of how science is done in order to avoid many of the problems described in this book. The two last contributions by Bauer and Castro Perelman reveal both a pessimistic view of the miseries of the actual system with a glimmer of hope in promoting ethics in science.

Of course, there is a third position which is not included in this book, which consists in saying that science has no major problems at all and the dissidents or scientists with critical ideas about the system are simply bad scientists with wrong points of view. Are the authors of this book mere charlatans who do not deserve any attention whatsoever about their complaints about the current system? There may be many strange, inconsistent or exaggerated points of view in the scientific contributions of some authors, and some points of view expressed here about the sociology of science might be misleading as well, but this is not a reason to avoid presenting and discussing them. All the authors elaborated this book with the honest hope to improve, to help, to analyse the system, with the sincere purpose to search the truth, both in nature and science as a human activity.

Many scientists were asked to contribute to this book but many feared all sorts of retaliations, like loss of funding and jeopardy to future tenure positions; many feared shame and ridicule from their peers; some very prominent ones would have contributed if we had found a front-line publisher; while others thought it was an utter waste of time for they believe it is hopeless, by definition, to try to change the way science is done nowadays because it involves humans. By offering this book to the readers we hope to find a middle ground between what may be perceived as the lesser of two evils: a young pessimist versus an old optimist.

The purpose was to collect miscellaneous contributions of diverse authors in order to avoid sectarianism and to promote criticism from different points of view. A minimum level of quality was ensured because all of us are scientists with experience in professional research and/or University departments rather than amateurs. Many of the coauthors are well-known and highly reputed dissident scientists with a very long

experience in battles against dominant paradigms. We have not tried to separate the different kinds of contributors, although we are aware that most of the challenging heterodox ideas have high probabilities to be incorrect. Even if somebody is wrong, there should be some outlet where one could express his/her complaints about the current state of science and the system which breeds it. The question here is not whether somebody has correct or wrong ideas about Physics and Astronomy (history will tell and not necessarily the present orthodox publications) but to analyze the problems of the official mechanisms under which current science is being administered and filtered. Nobody should have a monopoly of the truth. Whether the present ideas are part of a revolutionary movement in science or not is something which cannot be judged from an absolute point of view. Judge for yourself!

Martín López Corredoira, Editor.

Carlos Castro Perelman, Co-editor.

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Challenging Dominant Physics Paradigms¹

by J. M. Campanario and B. Martin

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Abstract: There are many well-qualified scientists who question long-established physics theories even when paradigms are not in crisis. Challenging scientific orthodoxy is difficult because most scientists are educated and work within current paradigms and have little career incentive to examine unconventional ideas. Dissidence is a strategic site for learning about the dynamics of science. Dozens of well-qualified scientists who challenge dominant physics paradigms were contacted to determine how they try to overcome resistance to their ideas. Some such challengers obtain funding in the usual ways; others tap unconventional sources or use their own funds. For publishing, many challengers use alternative journals and attend conferences dedicated to alternative viewpoints; publishing on the web is of special importance. Only a few physics dissidents come under attack, probably because they have not achieved enough prominence to be seen as a threat. Physics could benefit from greater openness to challenges; one way to promote this is to expose students to unconventional views.

Physics has a reputation as one of the most highly developed and well established fields of science. Although there are many exotic-sounding theories at the research frontier involving strings, black holes and charm, the basic postulates of classic theories such as electrodynamics, relativity and quantum theory are seen as solidly established.

It is surprising to find, therefore, that there are many challengers to orthodox physics who offer critiques of conventional theories and present their own alternative formulations. Furthermore, many of these challengers are well qualified, with degrees, mainstream publications, positions at well-known universities

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and prizes including the Nobel Prize. Table 1 gives a few examples, listing only a selection of these particular challengers' achievements. This is not a ranking of dissidents; there are others with just as many accomplishments.

Table 1. A sample of well-qualified challengers to orthodox physics

Halton Arp is a professional astronomer who has worked at the Mt. Palomar and Mt. Wilson observatories. He has received the Helen B. Warner prize, the Newcomb Cleveland award and the Alexander von Humboldt Senior Scientist Award. He has published a large amount of evidence that contradicts the big bang (Arp 1987, 1998).

Andre Assis is professor of physics at the University of Campinas, Brazil, is the author of several books and over 50 scholarly articles and is a leading authority on Weber's electrodynamics. He is a critic of relativity (Assis 1994, 1999).

Robert G. Jahn is professor of aerospace science and dean emeritus of the School of Engineering and Applied Science at Princeton University and has received the Curtis W. McGraw Research Award of the American Society of Engineering Education. He researches mind-matter interactions.

Paul Marmet was professor of physics at Laval University, Québec, for over 20 years, is author of over 100 papers in electron microscopy, was president of the Canadian Association of Physicists and has received the Order of Canada. He is a critic of relativity.

Domina Eberle Spencer is professor of mathematics at the University of Connecticut and has published several books and over 200 scholarly articles. She supports an alternative theory of electrodynamics, in the Gaussian-Weberian-Ritzian tradition.

Tom Van Flandern has a PhD in astronomy from Yale University, became chief of the Celestial Mechanics Branch of the US Naval Observatory and received a prize from the Gravity Research Foundation. He is critic of theories of the big bang, gravity and the solar system (Van Flandern 1993).

If you decide to question a widely accepted theory, or to present data that is anomalous in terms of current understandings, it can be difficult to gain a hearing. Although the essence of scientific advance is going beyond current knowledge or offering a new way of understanding data, questioning fundamentals is seldom welcome. Some types of challenge, such as perpetual motion machines or causality violation, are automatically rejected. Others, such as cold fusion, are openly considered and tested but then, if they do not measure up, henceforth rejected by mainstream science.

It is easy to dismiss challengers as "cranks," but this risks rejecting fresh ideas from those who are well placed to achieve radical breakthroughs. There are instances where the official expert view is later revealed as unproductively dogmatic, as when the French Academy rejected observations by common people of stones falling from the sky. It may be that "the kinship of the scientific crank with the scientific creator is more than a superficial one" (Watson 1938, 41) but few scientists embrace this connection.

A proponent of an unorthodox idea is likely to encounter several types of difficulties. First, it is difficult to obtain funding: very few research grants are awarded for proposals to re-examine long accepted theories. Most funding agencies expect that proposals will build on existing science rather than challenge basic postulates. Second, it is difficult to publish in mainstream journals. Third, proponents of unorthodoxy may come under attack: their colleagues may shun them, they may be blocked from jobs or promotions, lab space may be withdrawn and malicious rumors spread about them. Even if they can overcome these problems, they have a hard time gaining attention.

Our focus here is on strategies used by challengers to overcome such obstacles. In the next section we outline ideas from the social studies of science that help to explain the way science responds to challenges. Then, drawing on responses to questions we submitted to dozens of physics dissidents, we look at methods used by challengers to current paradigms to obtain funds for research, publish their work and survive attacks. We conclude with some observations about how challenges to orthodoxy, even though most of them are judged wrong, can be used constructively.

Understanding challenges

Of the large body of research in the history, philosophy, psychology and sociology of science, we here pick out a few key ideas that are helpful for understanding why challenges to orthodoxy are likely to be given a cold reception. We have found that some earlier ideas, now superseded in the eyes of many, remain useful for explaining challenges and responses, though for other purposes these same ideas have important limitations.

The most common view about how science works is that new ideas are judged on the basis of evidence and logic: if a new idea explains more data or provides more precise agreement with experiment, this counts strongly in its favor.

Karl Popper claimed that science advances by falsification (Popper 1963). In his view, it is the duty of scientists to attempt to disprove theories, confronting them with experimental data and rejecting them if they do not explain the data. Theories that cannot be falsified are, according to Popper, not scientific. Many scientists believe in falsificationism.

These conventional views were challenged by Thomas Kuhn (1970). Kuhn argued that scientists — and physicists in particular, since most of his historical examples were from physics — adhere to a paradigm, which is a set of assumptions and standard practices for undertaking research. If an experiment gives results contradictory to theory, then instead of rejecting the theory altogether, alternative responses include rejecting the experiment as untrustworthy and modifying the theory to account for the new results (Chia 1998; Chinn and Brewer 1993).

When anomalies accumulate, the paradigm can enter a state of crisis and be ripe for overthrow by a new paradigm. This process of scientific revolution does not proceed solely according to a rational procedure but involves social factors such as belief systems and political arrangements. Kuhn's successors have modified his model of paradigms and revolutions, for example showing that paradigms are not as well defined and

incommensurable as Kuhn imagined, but they have extended his insight that the process of scientific change involves social factors and is not just a rational matter (Barnes 1977, 1982; Collins 1985; Fuller 2000; Mulkay 1979; Pinch 1986).

In any case, the idea of paradigms puts a different spin on the problem of new ideas in science. Rather than being dealt with according to logic and evidence, challenging ideas may be ignored or rejected out of hand because they conflict with current models. In effect, the logic and evidence used to establish the paradigm are treated as definitive and are unquestioningly preferred over any *new* logic and evidence offered that challenge the paradigm. During periods of “normal science,” the ideas developed by mainstream scientists originate *from* current paradigms: they add more and more pieces to standard puzzles. Given that the paradigm is the source of ideas, it is not surprising that challenges to the paradigm — the framework that allowed mainstream scientists to contribute to the development of science — are seldom greeted with open arms. If a theory is not considered physically plausible, it may be rejected even though it makes successful predictions (Brush 1990).

Eminent philosopher of science Imre Lakatos says that research programs have a hard core set of fundamental principles surrounded by a set of subsidiary, less significant assumptions, called the protective belt. For the research program to advance, lesser assumptions can be tested and possibly modified, protecting the hard core from being falsified (Chalmers 1999, 130-136; Lakatos 1970).

Conventional science education helps to perpetuate current orthodoxy. Students are introduced to physics through textbooks that typically present current ideas as “the truth” and either ignore alternative ideas altogether or portray them as convincingly disproved by experiment. Students learn by solving problems, and the concepts and magnitudes used in these problems assume the validity of current theories. Only rarely are students presented with theories that don’t work, and even in those cases, such as Bohr’s model of the hydrogen atom, the intent is to show how researchers overcame problems. By and large, students are confronted only with success in science. Acceptance of received wisdom is deeper because orthodoxy is never discussed as orthodoxy: it is simply the truth. Students are also taught about the “scientific method” — observation, hypothesis formulation, testing, etc. — and hence come to believe that theories that have been tested by experiments are true, because the textbook scientific method is thought to be the way science actually operates. Views that science actually proceeds in a different fashion are seldom mentioned (Barnes 1974; Bauer 1992; Feyerabend 1975). Relevant here is a famous quote from Max Planck (1949, 33-34): “A new scientific truth does not triumph by convincing its opponents and making them see the light, but rather because its opponents eventually die, and a new generation grows up that is familiar with it.”

The system of examinations and degrees is a sorting process; the physics PhD screens out most of those who question orthodoxy (Schmidt 2000). Once students are committed to the basic principles of the field, then it is possible to begin research and to question, within implicit limits, prevailing ideas.

There are definite advantages to training and researching within a standard framework. Rather than spending lots of time getting bogged down in the basics, researchers instead can move more rapidly and confidently to the cutting edge, pushing out to unexplored areas of knowledge and, thus, reinforcing and developing the paradigm. As long as the basic principles of the field are sound, it makes sense to simply

learn them and build on them. Traditional education seldom tells students how to go about challenging current paradigms.

There is another obstacle facing challengers: the psychological commitment of scientists to current ideas, especially their own ideas and the dominant ideas. The usual image of the scientist is of a cool, calm, detached, objective observer, but the reality is quite different (Mahoney 1976; Mitroff 1974), as anyone who knows scientists is aware. The classic study of the psychology of scientists is Ian Mitroff's book *The Subjective Side of Science*, in which he revealed that Apollo moon scientists were strikingly committed to their ideas, so much so that contrary evidence seemed to have little influence on their views. As well, scientists express strong views, often quite derogatory, about other scientists. To expect every scientist to react coolly and objectively to a competitor's idea is wishful thinking, though there are some scientists who approach the ideal. Intriguingly, Mitroff found that it was often the top scientists who were the most strongly committed to their ideas.

Tom Van Flandern commented to us:

I have taken aside several colleagues whose pet theories are now mainstream doctrine, and asked quizzically what it would mean to them personally if an alternative idea ultimately prevailed. To my initial shock (I was naïve enough that I did not see this coming), to a person, the individuals I asked said they would leave the field and do something else for a living. Their egos, the adulation they enjoy, and the satisfaction that they were doing something important with their lives, would be threatened by such a development. As I pondered this, it struck me that their vested interests ran even deeper than if they just had a financial stake in the outcome (which, of course, they do because of grants and promotions). So a challenger with a replacement idea would be naïve to see the process as anything less than threatening the careers of some now-very-important people, who cannot be expected to welcome that development regardless of its merit. (1 August 2002)

Though it is easy to criticize dogmatism, a certain amount of it can be valuable for scientific progress. That was certainly Kuhn's view: unless the current paradigm was in crisis, dogmatism in science education and practice has a functional value (Kuhn 1963). Michael Polanyi, a chemist and eminent commentator on science, argued "that the scientific method is, and must be, disciplined by an orthodoxy which can permit only a limited degree of dissent, and that such dissent is fraught with grave risks to the dissenter" (Polanyi 1963, 1013). Similarly, Mitroff concluded that the classic norms of science, such as universalism, disinterestedness, communism and organized skepticism, did not adequately explain the operation of science, and instead proposed that counternorms were equally important, including "organized dogmatism."

Another problem facing challengers stems from the intense pressures under which most scientists work. Many scientists, especially those who are ambitious, work extremely hard. They may spend long hours in the lab or in problem-solving on top of other duties such as teaching, supervision and administration, not to mention life outside of work. Science is highly competitive and even the most talented scientists need to work hard to stay ahead of the game.

What happens when some challengers, who have spent years or decades developing their ideas, show up and ask a busy career scientist for an assessment? Even for an open-minded or sympathetic scientist, it is a

real sacrifice to spend days or even just hours examining alternative ideas, since that means correspondingly less time available for their own pressing work. The more eminent scientists serve as editors and referees for prestigious journals where they typically are focused on rejecting work that *fails* to meet the standards of orthodox science, making it even more difficult for them to accept work that *challenges* those standards.

Most challengers believe their ideas have value, otherwise they would not bother with them. What they desire from mainstream scientists is not acceptance (though that would be nice!) but a fair-minded examination of their ideas. There is a certain irony here: challengers confront academic power and what some of them see as corruption, but what they really desire is the attention of mainstream scientists. The practical problem facing challengers is a scarcity of attention: there are not enough scientists who have both the time and inclination to scrutinize their unorthodox ideas.

The way that science is organized exacerbates the problem of shortage of attention for paradigm challengers. Most scientists work as part of a small network, local and/or international, members of which address the same topic, share common interests and goals, exchange information and reprints, and attend the same conferences (Crane 1972). Scientists are more likely to devote attention to work by others in their network than they are to the work of outsiders. Dissidents who go to the roots of a paradigm do not specialize sufficiently to be part of such a network: they are outsiders in the field in the sense that they do not focus on a small portion of a paradigm. As a result, few scientists will be willing to give them any attention.

In summary, perspectives and evidence from the history, philosophy, psychology, and sociology of science, and from science education, suggest that the obstacles facing challengers are formidable. Most scientists, due to their education and day-to-day interactions, work within the prevailing paradigm. Most scientists develop a strong commitment to their own ideas, a psychological process that is reinforced by the large career investment in a particular line of work. Finally, the competitive struggle for success means that most scientists are extremely busy, with little time available to examine unconventional ideas.

There is, though, a contrary force: the rewards available for significant innovation. The founders of quantum theory and, above all, Einstein as the founder of relativity theory are heroes in physics for inaugurating new paradigms. Even short of these epic feats, physicists may aspire to be known for their contributions, which often means questioning the received wisdom.

Choosing research problems can be likened to an investment process (Bourdieu 1975, 1988). Scientists have available a certain amount of “capital” — knowledge, experience, time and effort — that they can invest in different ways. A conservative investment strategy is to pursue small, incremental innovations, with a high likelihood of success and a modest return on investment, following Peter Medawar’s dictum that science is “the art of the soluble” (Medawar 1967). A risky strategy is to pursue a speculative idea: the likelihood of success may be low but the returns, if the idea pans out, can be huge. For example, astrophysicist Fred Hoyle could be said to have originally invested in the steady-state theory of the universe, which had decent prospects but turned out to be a bad bet. He later made a riskier investment in the more speculative “life-in-space” hypothesis (Hoyle and Wickramasinghe 1978) which, if validated, would have more dramatic returns (though now too late for Hoyle). In a sense, paradigm challengers are ambitious

investors, in that they commonly criticize entire theories, such as relativity and quantum theory, rather than just a part of such theories. They seek to change theories at the level of university textbooks.

A different investment calculation comes into play, though, when it comes to someone else's ideas. To examine or even promote someone else's challenge to orthodoxy requires significant time and energy, yet the major returns go to the other person, if they are recognized as the innovator. If the idea is a promising one, the temptation is to grab credit, for example by domesticating the radical idea and publishing in orthodox journals. It is no surprise that many innovators are afraid of having their ideas stolen.

So although there is an incentive to pursue unorthodox ideas, only some researchers will be tempted to do so. The obstacles remain daunting, especially given that paradigm-challenging ideas are seldom taken seriously. Furthermore, few will have the eminence of a Hoyle to attract attention to their ideas.

Investigating dissent in physics

Our aim is to gain insight into how challengers can overcome the obstacles facing them. We began our empirical investigation by examining a range of work — including our own — on resistance to scientific innovation (Barber 1961; Bauer 1984; Campanario 1993a, 1993b, 1995, 1996, 1997, 2003; Mauskopf 1979; Nissani 1995; Sommer 2001) and suppression of dissent (Hess 1992; Horrobin 1990; Martin 1981, 1996, 1998, 1999a, 1999b, 2004; Moran 1998). From the large array of obstacles facing challengers, we concluded that three areas are of crucial importance: obtaining funding, getting published, and dealing with attacks. Though there are other types of obstacles, we focus on these three since our interest is less in obstacles than on ways of overcoming them.

By examining a diverse set of challenges, we came up with a list of ways of overcoming these obstacles. (See Table 2.)

Table 2. Some methods that challengers can use to overcome barriers to their work

1. Funding

- A. Obtain funding from innovative agencies.
- B. Obtain funding from agencies not worried about the innovative aspects.
- C. Obtain private funding.
- D. Fund the research through personal resources.
- E. Apply political pressure to obtain funding.
- F. Use conventional funding but disguise the nature of the research.

2. Publishing

- A. Challenge the editor's rejection.
- B. Use friends or patrons to help get published.
- C. Submit to other journals.

- D. Publish in many different journals and conferences.
- E. Keep publishing after the initial breakthrough.
- F. Seek wider audiences beyond the key discipline.
- G. Set up a journal or a special section in an established journal; attend alternative conferences.
- H. Send out preprints.
- I. Publish books.
- J. Publish paid advertisements.
- K. Seek coverage in the mass media.

3. Surviving attack

- A. Continue without being distracted or discouraged.
- B. Seek support from others who have come under attack.
- C. Expose the existence of attacks, especially their unscientific aspects.
- D. Expose the bias or vested interests of the attackers.
- E. Seek support from colleagues or a professional association.
- F. Counterattack using similar methods.
- G. Take legal action.
- H. Join with others who have come under attack.

To determine which of these methods are actually used in physics, we obtained the addresses of a sample of dissidents by means of webpages of journals such as the *Journal of Scientific Exploration* and meetings and societies of dissidents. To exclude most of the many uninformed and unsophisticated critics, we restricted our attention to those who have scientific degrees or are affiliated with reputable universities or have publications in mainstream journals, though no doubt this restriction excludes some worthy challengers. Given our aim of finding a diverse group satisfying our criteria — namely that they are challengers to dominant physics paradigms who have scientific degrees or research positions or publications — our search was extensive but not exhaustive.

We did not attempt ourselves to judge the quality of the dissidents' work. Whatever our own ideas about some research work's rigor, agreement between theory and experiment, quality of expression and the like, others would be likely to differ in their assessments, especially because judgments about quality are commonly mixed with views about whether conclusions are right or wrong. Hence, rather than use personal assessments in our selection process, we relied on the surrogate measures of degrees, affiliations and publications, which encapsulate the collective judgments of other scientists.

We wrote to a total of 41 well-qualified dissident scientists, mostly in physics, inviting them to describe their experiences in overcoming resistance to new ideas in science. We drew their attention to our list of methods (Table 2) but invited them to tell about any other methods that they had used. We obtained many fascinating responses, some of which are mentioned below. We did not seek to collect statistical data on use of different dissident strategies, because our aim was exploratory; responses to our letters were wide-ranging,

suggesting the limitations of imposing neat classifications at this stage. Not enough is yet known about dissident strategies to make it worthwhile pursuing quantitative categorization, especially given that self-reports may reflect different judgments about matters such as success and failure of a strategy.

Because our aim was to find out how contemporary challengers try to overcome obstacles, we ruled out those who were once dissidents but subsequently succeeded in obtaining recognition. There are a number of these who could be cited (Hook 2002; Hunt 1983), of which one of the most prominent is S. Chandrasekar (1969), whose ideas on stellar evolution were initially rejected by Sir Arthur Eddington and others. Examination of such cases suggests that most dissidents encounter the same sorts of obstacles whether they are ultimately vindicated or not.

How to mount a challenge

Although we asked about experiences in *overcoming* resistance, many respondents focussed more on the resistance itself, commenting critically on the nature of the scientific establishment. For example, Paul Marmet said that “Scientists prefer to stick to old theories even if they do not make sense. I was at first very surprised by that reaction but, after a few years, I had to admit that it is a normal human reaction” (28 July 2002). Ruggero Maria Santilli, president of the Institute for Basic Research, said “There simply is no way of correcting academic-scientific corruption and I consider futile any attempt at that” (4 August 2002). According to Bruce Harvey, a dissident physicist, “To say that the established scientific world is prejudiced against new ideas is an understatement. It is paranoid about them.” (13 August 2002).

On the other hand, some respondents believed that, despite resistance, in the long run their ideas would be recognized. David Bergman said that “Nevertheless, I am confident that the truth will come out and Common Sense Science will prevail as valid science. I have no ideas how long it will take, or how many will come to accept the scientific truth that modern physics must be replaced (not reformed).” (30 July 2002).

Funding

Innovators often have a hard time obtaining funding from conventional sources. Sometimes their funding is withdrawn. One option is to obtain a job in science — often by doing conventional research — and use it as a base to do unorthodox research. Those who are more successful using this strategy can even create a lab or institute, such as Princeton Engineering Anomalies Research, a laboratory for research on the mind-matter relationship. This option is more common for scientists who become interested in unorthodox ideas after establishing an orthodox career, as in the case of Brian Josephson, who won the Nobel Prize in physics for the discovery of the effect named after him and who is now working in parapsychology.

Sociologist Ron Westrum, who has studied the scientific community’s response to anomalous phenomena (Westrum 1977, 1978), thinks that the most comfortable basis for mounting a challenge to orthodoxy is as an older or retired professor (23 October 2002). Historian-of-science Stephen G. Brush offers similar advice: obtain a secure job and do conventional research to establish a reputation, thus laying a

foundation for proposing radical ideas (31 October 2002). However, these options are available to only some individuals.

Some pursue unorthodox ideas by using conventional funding but disguising the nature of the research. We are aware of a case in which astronomers, while using a major telescope for observations on a conventional research topic, used a bit of spare time at the end of their observing run to look for something different, relevant to an unorthodox theory. Richard A. Muller (1980) revealed how he circumvented the funding system for innovative (though orthodox) research. According to David Horrobin (1989), editor of *Medical Hypotheses*, scientists know that to obtain funding they must misrepresent their motivations in grant proposals, otherwise “All innovative scientists know that they would rarely get funded, such is the nature of the review system.”

Parapsychology researcher Helmut Schmidt — a physicist by training — was employed by Boeing Scientific Research Laboratories when he carried out some of his early work using quantum random number generators (Schmidt 1969). (Later he worked in a private research institute.) Corporate funding has sustained cold fusion research after it was rejected by mainstream science (Simon 2002).

There are a few grant-giving bodies open to unorthodoxy, such as the Lifebridge Foundation. Money for some types of unorthodox projects is available from the military, which does not want to miss potential applications no matter how unorthodox the theory behind them.

Other challengers do not ever get started in a conventional career. They are more likely to support their work through their own funds. This helps explain why so many challengers focus on theories; personal funds are seldom sufficient to sustain a significant laboratory. Cynthia Kolb Whitney, editor of *Galilean Electrodynamics*, says that “Personal resources have worked best for me. Though resources are modest, there are no discontinuities, uncertainties, interferences, or other annoyances” (17 August 2002).

Publishing

Innovators often have a difficult time getting published. Submissions may be rejected or subjected to significant delay. Major revisions may be required. Even when published, the work may be neglected. Challengers have used a variety of methods to promote their ideas.

Sometimes unconventional papers are rejected without refereeing or any critical comment, in which case the author can request a formal assessment. Apparently *Nature* previously returned all submissions from private addresses without looking at them; one of those so treated was atmospheric scientist James Lovelock, later best known for his Gaia hypothesis (Bond 2000). Authors also can contest the comments made by journals, asking for re-evaluation. Of course, challenging the editor’s rejection is a technique available to all scientists, but it is especially important when ideas may be rejected out of hand.

After a rejection, it is a standard technique to scout around to find somewhere else to publish. Challengers often have to search more widely in doing this. However, there is a down side, as indicated by Paul Marmet: “Spending too much time in an effort to publish our ideas in conventional journals leads to

serious frustration. That is a trap, which destroys the delicate ability that can lead later to new ideas.” (28 July 2002).

Stephen G. Brush recommends writing balanced review articles, with plenty of citations of other authors, for publication in a journal such as *Reviews of Modern Physics*, allowing the possibility of some self-citation (31 October 2002). However, we are unaware of any dissidents who have adopted this approach.

Even when challenging ideas are published, they may be ignored (Collins 1999). Therefore, publishing in a range of journals and presenting papers in a variety of conferences maximizes the chance that someone will take the ideas seriously.

Parapsychologists set up their own journals, rigorously refereed, such as *Journal of Parapsychology* — reputable enough to be included in the Science Citation Index database — to get around the low acceptance rate in mainstream journals. Other examples are *Journal of Scientific Exploration*, *Galilean Electrodynamics*, *Frontier Perspectives*, *Infinite Energy*, *Cold Fusion* and *Apeiron*. Several of our respondents reported favorably on alternative journals. Vladimir Ginzburg said he “published five papers in the journals that are receptive to speculative ideas, *Speculations in Science and Technology* and *Journal of New Energy*.” Caroline Thompson, who has challenged standard views on quantum entanglement, commented: “I attempted to publish my next important paper in *Physical Review Letters*. The story of its rejection is the subject of my Tangled Methods paper [Thompson 1999]. The paper in question has now been accepted by *Galilean Electrodynamics*. I have had other papers in *Infinite Energy* and the *Journal of New Energy*, and contributed chapters to a few books.” (16 August 2002).

Conferences dedicated to alternative viewpoints, and conference proceedings, provide a venue for challengers. Domina Eberle Spencer, who has worked since the 1940s on reformulating electromagnetic theory, reports that as well as new journals open to discussing fundamental questions, “International meetings which welcome discussions of such questions have taken place in St. Petersburg, Russia, Bologna, Italy, Cologne, Germany and Lanzarote, Spain. In the United States the Natural Philosophy Alliance was established in 1994 and has held annual meetings ever since.” Concerning her work, “Of course, it is still not possible to have these results recognized by the established physics journals. But the situation is much better than it was.” (27 July 2002).

In setting up alternative journals and conferences, dissident scientists imitate mainstream science. They may complain that orthodox science and conventional peer review reject their discoveries but they don’t try to develop alternative evaluation methods. Challengers are pleased when mainstream bodies organize conferences or conference sessions oriented to unorthodox ideas.

Some book publishers are more open to challenging ideas than journals, as long as there is a market. Hoyle and Wickramasinghe found publishers for a whole series of books. Self-publishing is another option, adopted by many dissidents. Vladimir Ginzburg reports that after rejections by publishers, he self-published three books. Chris Illert (1992/1993) self-published his work on a classical model for nuclear physics.

Publishing on the web is inexpensive and offers wide accessibility. Lars Wåhlin, who has developed alternative viewpoints on gravity and relativity, says, “I believe that it is better to publish on the internet because it will be available to everybody and not only to a few journal subscribers. It can appear for an

unlimited time and it has the advantage that one can make corrections at any time if necessary.” (4 August 2002). Bruce Harvey writes: “Failing to get my work on electromagnetic momentum immediately recognised by the British establishment, I produced my web-site. That led to my inclusion in many lists and invitations to fringe conferences. ... I have extensive email correspondence with others in the same field. ... I think my web-site amounts to a better exposure of my ideas than most professional scientists receive.” (17 August 2002). Paul Marmet says “Presently, the Web is by far the best compromise to publish new controversial ideas in science, because nobody can stop you, it is very widely distributed and it costs almost nothing.” (28 July 2002). In addition, there are some internet newsgroups devoted to “alternative physics,” such as the Natural Philosophy Alliance (<http://mywebpages.comcast.net/Deneb/npahome.html>) with its Dissident Physics Discussion Group: http://groups.yahoo.com/group/NPA_Dissidents/

Obtaining electronic publication in credible forums can be another matter. Caroline Thompson says that she “put copies of my papers in the archive arXiv.org. I was able to do this because I have managed to arrange to have a university address. Had it been otherwise I might have found it hard to register. A contact of mine who did manage to register from a home address and submit a paper was jubilant for a day or two then found his registration cancelled and the paper withdrawn.”

Another strategy is to publish a paid advertisement. For example, Pierre-Marie Robitaille (2002) paid to publish an article in the *New York Times*. Cameron Y. Rebigsol offers a \$50,000 reward to anyone who can disprove his mathematical arguments against relativity (<http://members.aol.com/crebigsol/awards.htm>). Such individuals are anxious to have their ideas scrutinized.

Seeking coverage in the mass media is another option. The mass media are not refereed but instead operate on the basis of “news values” such as prominence, proximity, conflict, timeliness, action, human interest, and perceived consequences. A scientific controversy, especially one involving a local personality, could well be considered worthy of coverage. However, most journalists are respectful of scientific authorities, so it can be difficult for challengers to obtain sympathetic coverage. One of the best opportunities for media coverage arises when unorthodox ideas are published in mainstream journals, as in the cases of parapsychology and homeopathy. On the other hand, many scientists look down on colleagues who obtain media coverage, so this option has disadvantages for those seeking greater credibility.

Surviving attack

Some innovators come under attack beyond normal criticism of their ideas. For example, their professional integrity may be challenged, malicious rumors may be spread about them, they may be threatened, their submissions or grant applications may be rejected without proper review, their grants may be removed, their access to facilities may be denied, and their jobs may be put in jeopardy.

Attacks are especially common when challengers provide support to a social movement opposing a powerful interest group, as in the cases of nuclear power, pesticides, and fluoridation (Martin 1999b). The most famous dissident physicist is Andrei Sakharov, known for his challenge to Soviet nuclear policy and for being a prominent scientist who was willing to speak out in a repressive society. Hugh DeWitt, a physicist at

Lawrence Livermore National Laboratory who was prominent in his criticism of US nuclear weapons policy, came under attack at several points in his career. Scientists and engineers critical of nuclear power have been suppressed in many countries (Freeman 1981; Martin 1986; Sharma 1996). These nuclear critics did not challenge physics paradigms, but the techniques used against them illustrate how paradigm challengers may be attacked.

Advice for whistleblowers emphasizes collecting large amounts of documentation of the problem to be exposed, consulting with family and friends before taking action, preparing to survive attack, not relying on official channels such as ombudsmen or courts, and carefully assessing options (Devine 1997; Martin 1999c). Some of these recommendations are relevant to physics dissidents. Before openly supporting an unorthodox idea, it is wise to collect documentation of good performance in one's job, be aware that there could be repercussions, talk matters over with family and friends and not assume that grievance procedures or professional associations will provide any help against harassment or victimization. It is unwise to risk one's career without being fully informed.

The experience of whistleblowers, from a range of fields, is that talking to other whistleblowers is immensely beneficial. The existence of networks of dissident scientists suggests the value of mutual support. Although dissidents often disagree with each other's theories—for example, some accept quantum theory but challenge relativity and others do the reverse—some of them are able to work together in societies like the Natural Philosophy Alliance. On the other hand, we are aware of dissidents who are quick to dismiss other dissidents as crackpots.

Only a few of the scientists we contacted described significant problems in their careers, for example having to leave university posts, due to their dissenting views. The response of an establishment to challengers typically follows the sequence of neglect, ridicule, attack and co-optation. Most challengers remain at the first stage, being entirely ignored. If they are ridiculed or come under attack, that is a sign of some success!

Cynthia Kolb Whitney, editor of *Galilean Electrodynamics*, says to ignore attacks. "People who make them will never be convinced anyway, so don't waste energy. Dissidents like us live in a parallel universe largely separated from mainstream physics, except when the big breakthrough comes, which it certainly will do from time to time." (17 August 2002).

One of the least effective responses is counterattack. Paul Marmet told of researchers who sued those who refused to accept their new idea. One won his court case after 15 years, but "after so many years, it was too late and he was no longer able to get new ideas in physics. He was just a legal expert." (28 July 2002).

Conclusion

Challenges to orthodoxy exist even in periods of "normal science," though this is ignored by most analysts of science. Many challengers are well qualified—with degrees, positions at reputable universities, publications in mainstream journals, even Nobel prizes—but their presence remains unknown to many scientists.

The life of a dissident is seldom easy. That is certainly the message from those who challenge dominant ideas in physics. Some have persisted in the scientific wilderness for decades.

Our impression is that most challengers believe in the scientific approach—that ideas should be tested on their merits, and that those ideas that work better will be accepted—sometimes more strongly than mainstream scientists. Roger Nelson says “I believe that it is essential to do excellent work” (13 August 2002). Ruggero Maria Santilli says “My main suggestion to fight established doctrines is that of bringing new theories to the level of predicting new demonstrated effects and then establishing them experimentally” (4 August 2002). Vladimir Ginzburg is “trying a new way of presenting a new idea. This way of presenting includes: a) clear and reasonable assumptions that are based on common knowledge, b) applying the commonly known calculation methods, and c) presenting the results that can readily be verified” (24 July 2002). Because many of the theories proposed by dissidents are comparatively simple and straightforward, they should be easier to falsify and therefore are, in Popper’s framework, exemplars of good scientific theories.

The experience of challengers, though, is that they are not treated “scientifically”. Instead, they are typically ignored or rejected without adequate examination. This also happens to many normal scientists but, because they are developing the paradigm, they cannot complain that unorthodoxy is the reason their work doesn’t receive attention.

Collectively, challengers have tried various methods to overcome the obstacles facing them, but few individuals seem to have carefully considered a range of options, much less tried them. There are, though, a few experienced challengers with well developed assessments of the dynamics of science and strong ideas about the best way to proceed.

Some mainstream physicists think that the way the discipline now responds to new ideas is just fine: the field is progressing, so why worry about those on the fringe? Most of them are wrong, so why bother?

But there is another viewpoint: challengers, even those who are wrong, offer a potential source of strength to science. Their incessant questioning can be used to guard against complacency, to improve thinking and to prop open the door to change. One of the greatest perceived strengths of physics is its openness to speculation at the cutting edge of research. The field is not so fragile that greater openness concerning established principles is a real threat to the achievements of the field, though it may be threatening to some whose careers are built on particular findings or theories. Greater openness to challenges would increase respect for the field from potential contributors, whereas dogmatism and arrogance cause alienation.

Teachers often say to their students that they should be skeptical, not believe something until it has been tested, and so on. If students later perceive that dissidents are ignored or their theories rejected without testing, that hurts the image of science, even when the dissidents are wrong.

But what does greater openness mean? Certainly not automatic publication of any dissenting idea. If physicists want to be more open to new ideas, the key is attention: spending some time examining unorthodox ideas. One way to achieve this is to give students—such as advanced undergraduates—projects that involve theoretical assessment or experimental testing of unconventional views. This will extend

students' minds. If good students cannot refute a challenge, then it might be time for attention by more experienced researchers. Other options are setting up new journals and websites for challenging ideas, and treating them seriously.

Another use of dissent, in teaching, is to show students what happens to those who challenge current theories. This should be a part of the curriculum at the university level to avoid misconceptions about how science works. It can also provide insight to dissidents who might choose, as Tom Van Flandern suggests, to "keep their heads down" so they "can survive long enough to become senior in their fields" (30 September 2002). Finally, for those who get their work published with no difficulties, studying the travails of dissidents can provide insight into what it is like for others.

Charlatans and others interested in exploiting the public's ignorance sometimes seek credibility by pointing to dogmatism in science. If scientists are seen to be open to new ideas, public confidence in science can be bolstered.

No one knows the optimum level of tolerance for new ideas in a field. This might be something worth experimenting with. Physics challengers would certainly say that, at least as regards their own ideas, tolerance needs to be increased.

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**The Gold Effect:
Odyssey of Scientific Research
by W. Kundt**

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Abstract: This Essay will sketch the early successes, and later pitfalls of my scientific life, as a person whose endeavour it has always been—at least in my memory—to help understand the grand design of physics. Beginning as a student of Pascual Jordan at Hamburg University, I was lucky to meet already at a young age a few of the world's greatest physicists: Unforgettable are a joint dinner with Wolfgang Pauli in Hamburg's Curio Haus, and a 20-minute conversation with Richard Feynman during a picnic near Warsaw in 1962, but also longer and shorter encounters with Hannes Alfvén, Asim Barut, Peter Bergmann, Hermann Bondi, Don Cox, Paul Dirac, Murray Gell-Mann, Thomas Gold, Fred Hoyle, Joseph Jauch, David Layzer, Phil Morrison, Ted Newman, Rudolf Peierls, Ed Salpeter, Irwin I. Shapiro, Viktor Weisskopf, Carl Friedrich von Weizsaecker, John Archibald Wheeler, Eugene Wigner, and many others. Deplorable were the shortness of my active directorship at Erice, and the event that made me cancel my membership of the DPG in 1998.

Becoming a member of the scientific community

How objective are physical sciences? When I made up my mind to become a physicist, at the age of twelve, in Dresden, during World War II, it was primarily the impression that physics was an objective human discipline, free from individual judgement, arbitrariness, or mood that influenced my decision. Physics was a gigantic puzzle whose correct, permanent growth was controlled by multiple redundancy in the structure of its constituent pieces which had to match perfectly to eventually yield the grand design. This impression survived all of my school years (1943-1950), in Dresden and later (after the bombing) again in Hamburg, also my ten years of study at Hamburg University (culminating with the granting of my Ph.D., in early 1959), under Pascual Jordan, Ernst Witt, Erich Bagge, Otto Heckmann, and a large number of other established mathematicians and physicists, all of whom conveyed the message that the natural sciences were an objective human endeavour.

This impression (of an objective field of study) still survived through my first five years of research activities, in General Relativity (GR) and Quantum Field Theory (QFT, 1960-65), aimed at gaining further insight into fundamental physics as well as gaining a professorship, in regular exchange primarily with Jürgen Ehlers and Engelbert Schücking, also with Peter G. Bergmann's group at Syracuse (N.Y., 1959-60), and with the large and qualified QFT staff associated with Willibald Jentschke's particle accelerator DESY at Hamburg-Bahrenfeld, among them Hans-Jürgen Borchers, Georg Süssmann, and Hans Joos. (Highlights during these five years were the building of our house in Hoheneichen, and an improvised gathering there, during the 1964 IAU meeting at Hamburg, with twelve of the world's greatest in my field, among them Hermann Bondi, Thomas Gold, Ivor Robinson, and Dennis Sciama). And it survived through ten more years as a young husband and father, lecturer, research fellow, and group leader at Hamburg, Kiel (1965), Pittsburgh (Pa, 1966), CERN (1972), and Bielefeld (1973), in collaboration with Klaus Hasselmann and Gerd Wibberenz, Ted Newman, Roger Penrose, Stephen Hawking, Bob Geroch, Hans-Jürgen Seifert, Rolf Hagedorn, Helmut Stichel, and finally with Hans Heintzmann (Cologne) and with Eckhard Krotscheck, my closest research associate during those years.

Those ten years, 1965-75, of happy family life, unburdened learning and intense research, saw me and my friends rise into an internationally respected science community, specialists mainly in GR but also, to some extent, in canonical quantization, in applied mathematics, and statistical mechanics, trying to test Einstein's theory at the 1% level with experiment 11 of the German-American space mission HELIOS (which approached the Sun to the distance of Mercury's orbit, 0.3 a.u., in an attempt to thoroughly sample the Earth's closest Galactic environment), and systematically shifting attention from pure physics through cosmology into its various applications to solar-system physics, and to astrophysics. I oscillated between Hamburg, London, Paris, Princeton, Los Angeles, Geneva, and Bielefeld, to name a few. Supported by letters of recommendation from John A. Wheeler and Felix A.E. Pirani, CERN accepted me as a one-year Visiting Scientist in 1972, to help fuse particle physics with cosmology, perhaps via Rolf Hagedorn's highest temperature (of 10^{12} K). And I was invited, more than a year ahead of time, for the opening lecture on cosmology of the 1972 joint assembly of the EPS and the Astron. Gesellschaft in Wiesbaden, in the main auditorium of the Rhein-Main Hall, recommended by Evry Schatzman. And all of my submitted publications went straight into print. These were fruitful years; science had become my passion.

Not all hurdles had been taken without scratches: At the end of 1964, I offended the faculty of Hamburg University's Physics Department, represented by DRNS ('Döring, Raether, Neuert, and Stähelin'), by handing in a habilitation thesis which was incomplete. A few pages were missing, appendices between the main text and the list of references, which Prof. Jordan's private Secretary had not managed to finish typing in time for the planned deadline. The thesis covered a lot, twice as much as required for the strived-for degree (in my judgement), and I was reluctant to see its submission unduely delayed, for no cogent reason. I could have cut out the untyped pages altogether, including the references to them in the text, and restored all of them later for the published version; but that would have been far less economical.

Unaware of academic manners, I handed in the fourteen missing pages after the Christmas holidays, early in 1965, bandaged with green ribbon: My thesis had passed through the hands of the faculty

incomplete, without official permission! That clearly had been an offense, I was to learn. But they treated me kindly: All I had to do was ask formally to retract my application (for habilitation), to which request they consented, and submit a new application one term later. This re-submitted version of my thesis was printed in the *Springer Tracts in Modern Physics* Vol. 40, 107-168 (1966), under the title 'Canonical Quantization of Gauge-Invariant Field Theories'.

When (my former lecturer) Erich Bagge heard of this decision, he could not believe it, and invited me as a guest professor to Kiel University, unhabilitated, to lecture two terms of Quantum Electrodynamics. These happy (for me, perhaps not for the students) lectures were prepared during weekly commutations via S-Bahn, Hochbahn, train, streetcar, running, and/or taxi, back and forth between the Kiel campus and Hamburg Hoheneichen, where I hoped to meet Ulrike.

My habilitation thesis contains an erroneous lemma, on page 124, as Arthur Komar once communicated to me by airmail; I agreed by return mail. It was hopelessly wrong, as I would have noticed immediately had I tried to communicate it to any congenial person: For Lie algebras, ideals defined by single (Lie) multiplication differ from those defined by iterated multiplication. Apart from this error, the thesis failed to indicate a way how to quantise GR, in a consistent manner. Today I think that GR must not be quantised, already because there is no single physical effect that would be able to test it. This conviction, together with a plausible alternative approach, has now appeared in *Foundations of Physics* **37**, No. 9, 1317-1369 (2007), under the title of 'Fundamental Physics'.

Ideal working conditions

Perhaps the most active years of my academic life were now to follow, 1975 to 1985: Wolf Priester had convinced me to trade my lonely H2 professorship at Hamburg University's first Institute of Physics, at the end of 1977, with a C3 professorship at Bonn's Institute for Astrophysics, associated with his directorship, where my research work was as liberal as possible, stimulated by weekly colloquium talks (organised jointly with Bonn's Max-Planck Institute for Radio Astronomy, whose directors were Otto Hachenberg, Peter Mezger, and Richard Wielebinski), and by regular, most friendly conversations and discussions with him and younger colleagues in the house. Wolf Priester soon transferred the guidance of his weekly 'Tea Seminar' to me, which is still running now, essentially uninterrupted, after almost thirty years.

Starting with the key topics of spaceflight, solar-system physics, quasars, neutron stars, and black holes, the Tea Seminar soon expanded to cover all related problems in astrophysics: supernova remnants (SNR), supernovae (SN), cosmic rays (CR), gamma-ray bursts (GRB), jets (or bipolar flows, BF), young stellar objects (YSO), SS 433, the Ly-alpha forest, accretion disks, stellar binarity, stellar winds, atmospheric superrotation, magnetic dynamos, in-situ acceleration, pair plasmas, high-velocity clouds (in the Galactic halo, HVC), our Galactic Center, the merging (and/or harassing) of galaxies.

These subjects were successively studied in collaboration—often quite animated—at first with Eckhard Krotscheck in Jungiusstrasse 9, and subsequently with Marko Robnik, Axel Jessner, Reinhold Schaaf, Hsiang-Kuang Chang, Hajo Blome, Hans Baumann, Carsten van de Bruck, and Gernot Thuma, my disciples

'auf dem Hügel' 71, in Bonn Endenich. These topics will recur time and again in the following four sections, jointly with dozens of associated ones, all of which I hoped to combine into one consistent description of our cosmic neighbourhood—a hope that turned out to polarize unintentionally my relationships with a number of internationally leading scientists.

For a more precise description of the scene, I should mention two short, but honourful and important invitations abroad: to Cambridge (England), by Martin Rees, in the summer of 1977, and to Kyoto (Japan), by Humitaka Sato, in the spring of 1978. During these two invitations, I met a large number of influential astrophysicists, learned about their achievements and beliefs, was often confused, tried to fully understand the Crab Nebula, and the Central Engines of galaxies—Black Holes or otherwise—including Sgr A*, plus many further problems that ranked around them. Martin had been my external astrophysics teacher throughout a number of years, via his various publications, but could not convince me of his newly adopted black-hole favouritism. And Humitaka, together with his bright Ph.D. Tetsuyo Hara, were ideal partners for friendly discussions.

I thus plunged into international research, trying my best to deal with its most difficult and topical problems, yet unaware that not everybody is invited to correct other people, in writing, when he does not easily follow their conclusions.

Conflicting opinions, or the NIH effect

My first encounter with the way research can be handled when it tries to correct earlier work occurred in a paper on neutron-star crust matter, submitted jointly with Eckhard Krotscheck and Hans Heintzmann, which we proudly sent to *Annals of Physics* in April 1975. We had tried to apply the elasticity theory by Gordon Baym, David Pines, and Jakob Shaham to the noisy behaviour of neutron-star pulse-arrival times, both (discrete) glitches and (quasi-) continuous noise, and noticed that it was inconsistent with our understanding of neutron-star matter: Its pressure, exerted by Fermi-degenerate electrons, can be orders of magnitude higher than the shear modulus of its ion lattice, so that the crust behaves to first order like a liquid, to higher order like a jelly, and standard elasticity theory cannot be applied. Instead, we solved the new problem, and concluded that it predicted continuous noise as an aftermath of discrete glitches. This paper was received by AOP on 9 May 1975, and declared "lost" two years later, with no warnings in between. Heintzmann spoke of an NIH effect: "Not Invented Here". Not much later, Gordon visited us at Hamburg, and I met Jakob on several other occasions, both of them quite friendly; at Kyoto, David happened to visit for a few days whilst I was there.

Again in 1975, I could not follow Hawking's (published) claim that a newly formed stellar-mass black hole would have a gigantic entropy, orders of magnitude larger than that of (the rest of) its host galaxy. I had known Stephen for many years, with encounters (invitations) at Hamburg, Cambridge, Cargèse, Princeton, and elsewhere, and he had kept me permanently busy learning from his quasi-regular preprint output; we were friends. So when I discussed his new result (on BH entropy) with Hermann Buchdahl, it occurred to me that the definition of BH entropy he used was different from what astrophysicists would call it. The compact

remnants of burnt-out stars, white dwarfs and neutron stars, have increasingly smaller entropies with increasing compactness, and the final step to a BH should be similar. His quantity measures the entropy of an evaporated BH, produced during a gigantic interval of time, not that of a newly formed one. This conviction appeared in print as a Letter to Nature, early in 1976, with more than half a dozen typos, and has been largely ignored (except by Werner Israel), even though all my relativity friends to whom I mentioned it agreed with me. Still at present days, string theorists are proud when they can derive Hawking's gigantic number. During my two subsequent encounters with Stephen, when I asked him for his reaction to my Letter, his answer was unintelligible to me (and to our interpreters). Our communication ceased. And I decided to leave GR, my distinct working field for over ten years, in favour of more testable physics.

Two further controversial items emerged in 1976: On 26 March, *Nature* received a Letter from me, and so did *Physics Letters*. The first letter dealt with SN explosions: How can an evolved star eject its envelope when its core collapses? The second one dealt with binary neutron stars, whose corotating magnetospheres interact with the wind from a nearby companion star. Neither of the two is such that I would still defend it literally today, against a critical examiner; both of them experienced several later updates, in new publications, until few years ago. The difficulty consists in suitable analytical approximations, of complex 3-d MHD scenarios which are not easy to evaluate quantitatively. But in both cases, I am still convinced, today, of being much nearer to reality than the alternative answers by others.

More in detail, my 1976 approach to the SN problem conflicts with Shklovskii's 1962 solution, when he treated a SN as a thin-walled bomb, via a Sedov-Taylor shock wave, instead of as a thick-walled (splinter) bomb, ejected by a light-weight piston. In order to transfer the gravitational collapse energy to the star's envelope and eject it, at more than 1% of the speed of light, such a piston must be extremely relativistic, because it would otherwise lose its radial momentum much too early on the runway, via adiabatic expansion, its temperature T dropping as $1/r^2$ with radial distance r . In above Letter I propose a toroidal 'magnetic spring' (or spiral) as the piston, forcedly wound up during the collapse (by conservation of angular momentum). Since 2003, I prefer, in addition, the spring's decay product, an extremely relativistic cavity [Kundt, 2005]. None of the (international) numerical approaches of this problem have so far succeeded in modelling a realistic piston, by 2006. They have failed to achieve ejection, often without admitting their failure. Deplorably, this situation has led to an open fight in 1988 (with my former friend Sam Falle, from Leeds), and to multiple applications of the methods mentioned in the following section, against the spirit expressed by Richard Feynman [1986].

The other Letter of 26 March 1976 had been stimulated by my then great friend Ed van den Heuvel (from Amsterdam), during a visit at Hamburg. It was his observation that most of the (then known) binary X-ray sources had much longer spin periods than their (solitary) pulsar cousins. Had they been spun down, in interaction with some wind material from their companion (he asked me)? I liked his suggestion, in particular as a potential source of the mysterious cosmic rays, and also as an important fundamental theoretical problem: how strongly does a rotating magnet interact with an ambient plasma cloud? In repeated publications between then and now, one of them in collaboration with Nigel Holloway and Yi-Ming Wang, I convinced myself that this magnetospheric torque must not be underestimated, and that it depends critically

on the depth of penetration, which may fluctuate strongly with time. (This problem has re-emerged, years later, in connection with the magnetars). I now think that all the high-energy cosmic rays, up to their (observed) record of $10^{20.5}\text{eV}$, are boosted by Galactic neutron stars, predominantly in hardness by the newborn pulsars, and predominantly in number by the dying pulsars (of typical spin-down age $10^{6.4}\text{yr}$), in interaction with a surrounding low-mass accretion disk; whereby sawtooth-shaped magnetospheric oscillations can lead to quasi-periodic relativistic slingshot ejections, remotely reminiscent of sparks from a grindstone.

Shortly before Christmas 1976, Ed was kind enough to advertise my 'magnetic propeller' approach—not to be confused with the (much more weakly torquing) stationary result by Illarionov & Sunyaev [1975] who had introduced the 'propeller' terminology—at the Texas Symposium in Boston, where he was strongly opposed by the brothers Fred & Don Lamb. He later worked on an improvement of my approach, after a consultation by Martin Rees, which had problems, and finally gave up on this item (as far as I know). Unfortunately, for no reason I could see, my early friendship with Ed gradually decayed, even in spite of a most hospitable, sunny joint invitation to the 1993 Bodrum September school by Nihal Ercan.

Perhaps my most controversial astrophysical object has been, and still is, the black hole; have we detected any BH in the sky yet? Or had John Wheeler and Remo Ruffini (in 1971) accidentally introduced a name for something that will never be found? Is Cyg X-1 one of them? This question was hotly debated during the 1972 summer school at Varenna where theorists and observers, mathematicians and phenomenologists forgot their lunches and sunlit Lake Como in order to find convincing, incontrovertible evidence in favour of, or against, this possibility. In both 1973 and 1976, *Physik in unserer Zeit* published articles by me on the expected properties of BHs, (which I still sign); and when Ed van den Heuvel (again!) got me alerted over dinner, at Hamburg in 1975, with the frivolous statement that he could hear a neutron star tick inside of the Cyg X-1 system, I accepted his bet (over a bottle of wine) if one of us could convince his partner wrong within one year, of the presence, or not, of a BH therein. I tried to defend my own brainchild, the BH.

That one year passed, without ultimate evidence. Ed had not accepted the Auriemma et al Letter to *Nature* [1976] on optical 83.53-ms-pulsations of Cyg X-1 as a proof of his victory, because he did not trust it; and I had become suspicious of my former belief: there was too much variability in the system, without a neutron star (with a variable, transverse magnetic moment). Why not allow the accretion disk around the neutron star to grow to a weight of a few solar masses, at electron-degenerate densities, looking like a dark, heavy mass? In April 1979, I submitted this interpretation of Cyg X-1, as a Letter to *A&A*, and received a hand-written answer from the anonymous referee: Ed van den Heuvel (who else)! He and Jerry Ostriker were not happy with my presentation, but did not see how to shoot it down either, hence let me publish—after an inclusion of its stability analysis based on Tassoul's book—without ever liking it. The BH showed its teeth ...

During my year of losing faith in the stellar-mass BHs, 1976, I simultaneously lost faith in their supermassive cousins, supposed to lurk at the centers of galaxies. For years, Martin Rees and Franco Pacini had coexisted amicably at conferences, describing those monsters as either BHs, or Spinars: fast-rotating, supermassive stars. But at the 1976 Texas Symposium, Martin had shifted his odds exclusively to their

expected stable end products, BHs. When I arrived at Cambridge for an extended period, in the summer of 1977, and enjoyed a common lunch with him, I asked him whether we could write a joint paper on Sgr A*, the central engine of our Milky Way; he ignored my question. Obviously, I had asked it at the wrong moment. I received a written answer from him in his [1984] review paper with Mitchell Begelman and Roger Blandford on the bottom of page 14 where they apologize for omissions from their more than 800 references by writing: "We have endeavoured to emphasize more recent (than 1975) theoretical papers that are written within the jet / massive black hole framework at the expense of work describing alternative models of radio sources....We apologize to those colleagues whose work is either omitted or misinterpreted." This answer, at the same time, covered Martin's omission of my [1980] jet Letter to *Nature* with Gopal Krishna in his Albuquerque review talk (1981), a paper whose contents strongly overlapped with Phil Morrison's unpublished evening (antimatter) lecture at Socorro, during this same meeting.

In my [2005] Springer book *Astrophysics, a New Approach*, I offer a number of reasons for why none of the proposed BH candidates in the sky can be black holes, both of stellar mass, and supermassive; the hurdles to their formation are not easily taken. See also [Kundt, 1998].

My life-long directorship at Erice, which lasted two years

One day during 1984, I received a telegram from Antonino Zichichi in which he invited me to attend two meetings on "Underground Physics", 25-30 April 1985, the first at St. Vincent, the second at L'Aquila. My atlas told me that St. Vincent lay near the southern end of the West Indies, whilst L'Aquila lies east of Rome, at the western edge of the Appenin mountains. Above time interval straddled a weekend, hence during the jet age, such a meeting sounded feasible. I had known Nino since my 1972 year at CERN, but had lost sight of him, and I occasionally regarded my struggling with organizers and publishers to be of the "underground" type, hence I accepted his invitation cheerfully.

It subsequently turned out that there was another St. Vincent in the Alpine Aosta valley, gorgeously constructed several hundred meters above the bed of the river, so that the weekend transfer from there to L'Aquila was scenic, and could proceed via buses. It also turned out that a fanciful popular-science article which I had written at Erice during the 1984 Course of Maurice M. Shapiro, on Zichichi's general request, and had not heard of again, had served, so to speak, as a written test for him, because over dinner at L'Aquila, he asked me to become one of his directors at Erice. We invented "Neutron Stars, Active Galactic Nuclei, and Jets" as the title of my school, with a purposely wide astrophysical scope; the financing had to be done through NATO money, as e.g. routinely for M² Shapiro's parallel school.

My 1984 *Il Nuovo Cimento* article was posted at the Erice Center when I arrived there, in September 1986—with Peter Scheuer as my co-director—verifying my 'examination' conjecture. Another benefit of Nino's invitation was Larry Sulac's inviting me to Boston University after my talk at St. Vincent, for the spring term of 1986. During which guest professorship our daughter met her future husband. Needless to say, I liked the challenge of my new appointment at Erice, if only for two weeks every two years, and had

decided to split it evenly between "Astrophysical Jets and their Engines" and "Neutron Stars and their Birth Events", hopefully.

It did not happen. Some higher intelligence turned my application to NATO (for funds) down for the second Course, in September 1988; it had to run on a largely reduced financial scale, i.e., with fewer lecturers and students, involved higher (voluntary) teaching loads, and provoked a few unpleasant intermediate scenes. When my third application to NATO, for a Course in 1990, was again turned down, with a skillful avoidance of Zichichi who had meanwhile become a member of the selection committee, I recognized another NIH effect in my scientific career.

During my life, I have organized some 15 meetings: at Hamburg, starting in 1968, to celebrate Pascual Jordan's 65th, 70th, and 75th birthdays, at Kamen's summer school for students in 1973, at Bielefeld in 1974, at Mönchberg (on pulsars) in 1975, at Erice in 1986 and 1988, for my retirement at Bonn, in 1996, and some six further small conferences at our preferred German meeting place in Bad Honnef (near Bonn), between 1977 and 1998. Most of these meetings, or workshops, were highlights in my life, both scientifically and socially, and five of them are documented in printed books. Only two of them involved unpleasant intermittent scenes: Erice 1988, and Bad Honnef 1998. In these two cases, I may have attempted too much, overpacked the schedule. Or I may have underestimated the Gold effect, to be explained in the last section. It is not always easy to swim against the tide. Or, as G.C. Lichtenberg once put it, in 1780: "Es ist unmöglich, die Fackel der Wahrheit durch ein Gedränge zu tragen, ohne jemandem den Bart zu sengen." (It is impossible to carry the torch of truth through a crowd without scorching somebody's beard).

For the preparation of some manuscript, I once received in the mail a double-paged printed sheet marked "Springer-Verlag LATEX A&A style file 1990", and dated 12.6.1990. Its title reads "Magnetohydrodynamic shock waves", and its authors read "N. Copernicus, G. Galilei, E.P. Hubble, I. Newton, and C. Ptolemaeus", from institutes at "Zielona Gora, Torino, Havard-Smithsonian Center, Armagh, and Zografos (Athens)", whereby offprint requests should be sent to: "E.P. Hubble, on leave from the Max-Planck-Institut für Radioastronomie, Auf dem Hügel 69, D-5300 Bonn 1, Federal Republic of Germany". The manuscript contains nine proper-looking tensor equations, a bit reminiscent of Hamburg's relativity group, and the long list of 67 references contains not only the classic Blandford & Rees 1974, a joint IAU circular by Galilei & Copernicus 1984 on Astrophysical Jets, and a 1987 preprint by J.G. Kirk, R. Schlickeiser & P. Schneider, but also a (properly quoted) Kundt, W. 1987, in: *Astrophysical Jets and their Engines*, ed. W. Kundt, Reidel C 208, p.1, a (typically misquoted) Kundt, W., Krotschek, E. 1982, *A & A*, 83, 1, and a Scheuer, P.A.G. 1987, in: *Astrophysical Jets and their Enigines*, ed. W. Kundt, Reidel C 208, p.129. To this day, I do not know the author(s) of this style file sheet. It could have been conceived at the time of my 2nd rejection by NATO. I was surprised then, on receipt, at the deep scientific and historical insight of the author(s), and I am still surprised today that the sheet passed essentially unechoed. Was it only sent to me, not to hundreds of authors? Was it meant to tell me something? Was I being pitied? Should I keep my convictions more to myself? Or was it just the joke of a personal friend?

Missing quotations, omitted invitations, and rejections by anonymous referees

Starting as an assistant of Pascual Jordan at Hamburg, and continuing as a C3 professor at Bonn, I have been regularly confronted with good and bad literature. Was there always an objective dividing line between serious and crack-brained? I think there was. Certainly, even a genius can fail on some important problem, at least temporarily, and even an educated university professor can continually publish erroneous calculations and views. A strict division is not always simple. Problems like the role of quantization in physics have even kept groups of the world's best physicists locked up in opposite camps for decades, quarrelling with each other. In order to keep the extended literature clean from insane injections, respectable journals screen their editions by *peer reviewing*.

Such peer reviewing, in my view, can be a nuisance, in particular when anonymous: Historically, there are many examples of prize-winning publications which had been rejected by peers, and were saved by helpful insiders. When I was young, from 1976 until 1980, several of my subversive 'alternative' ideas went straight into *Nature*, as Letters, partially under 'Matters Arising', to alert the community. Thereafter, *Nature* closed its weekly issues to me, even though I can remember a long and most congenial visit of (its Editor) John Maddox at my Bonn office. A second planned visit, unfortunately, coincided with a trip abroad of mine.

As another refereed journal for astrophysical publications, from 1977 until 1987, I enjoyed our European *Astronomy and Astrophysics*, unless publication times consumed up to three years, (even when its editor, then Michael Grewing, tried to help us). Alternatively, I was happy to sneak into the disputed (unrefereed?) journal *Astrophysics and Space Science*, from 1979 until 1998, whose editor Zdenek Kopal became a personal friend of mine, exclusively via letters. Zdenek published so fast, by using in-house referees, that I learned to have my submissions screened 'at home', by critical friends, before putting them into the mail.

Those happy years allowed me to publish on galactic centers (without BHs), purely leptonic twin-jets (performing $E \times B$ -drift), SS 433 (without bullets), Stockert's Chimney (coined by Ulrich Mebold), our Galactic Centre, cosmic-ray production, (magnetized) accretion disks, cometary plasma tails, jet-formation by newborn stars (with Hajo Blome), solar oscillations (with Hans Volland), pulsar nebulae, an ISM composed mainly of pair plasma, (Galactic) gamma-ray bursts (with Hsiang-Kuang Chang), pulsar theory, the youngest known Galactic SN (in Orion), and planetary superrotations (with Gunnar Luettgens), undelayed and unburdened by nonessential worries, even at the risk of not reaching the majority of my colleagues. Whereby after Zdenek's death, in June 1993, John Dyson continued his work masterly for another five years, by finding the proper referees for me.

Those of you whose manuscripts were repeatedly rejected, by anonymous referees, can understand me: Each time, you feel like you are punished, you feel personally hurt. In particular when you are not given a chance to reply, to argue it out with the other person. And you feel equally offended when you are not admitted to conferences, because you cannot see the cogent proof of a source being a black hole, or of two nearby galaxies being about to merge, (instead of, perhaps, only harassing each other), or because you cannot sign a numerical approach to the SN problem which leaves out an important 'detail', or the assumptions that

go into other people's jet models. It hurts when, for such reasons, you are even sent home from a conference, by someone who formerly had been your declared friend.

I felt specially disappointed when a short review of my [1998] birthday book *Understanding Physics*, edited by my first diploma student and personal friend Arne Richter, was rejected by the new editor from appearing in the *Physik Journal*. Why did I deserve this treatment, as a decades-long faithful member of the DPG, and a friend of several of its past presidents? I cancelled my membership.

Reliability of front-line physics, or my growing list of alternatives

Thomas Gold was a close friend and teacher of mine. His scientific life has been quite combative, already during the years when he cautioned NASA's manned space research, as a director of the Cornell Center for Radiophysics and Space Research, and later as a consultant of the Swedish government to dig for petroleum from the Siljan Ring impact structure. Perhaps his strongest insight into our planetary structure is superbly presented in his [1999] book *The Deep Hot Biosphere*, in which he argues that most of the fossil fuel reserves are of abiogenic origin, and that life on Earth probably sprang forth in (cracks of) its solid crust—not in its primordial oceans—thermally hosted and sheltered against the heavy interplanetary bombardment, energized by rising natural gas, and (partially?) catalyzed by clay. His deep insight into physical mechanisms is best expressed in his (unpublished) saying: "Every complex physical problem has at least one simple, intuitive, and well presented wrong solution". (A perfect recent example is the distance re-determination of SS 433 in [Blundell & Bowler, 2005]).

The term 'Gold Effect' was coined by Raymond Lyttleton in [1981], after a conversation with him during which Gold had explained how a mere unqualified belief can occasionally be converted into a generally accepted scientific theory—a dogma—through the screening action of refereed literature, of meetings planned by scientific organizing committees, and through the distribution of funds controlled by 'club opinions'. In the (last) chapter *Cargo Cult Science* of his book *Surely you're Joking, Mr. Feynman* [1985], Richard Feynman gives lucid examples of the same phenomenon. Quite generally, it occurs to me that any non-trivial physical result—in astrophysics, geophysics, biophysics, or elsewhere—which has not benefitted from redundant experimental tests has a high chance of being wrong: physics is not all that easy.

In this collection of episodes, encounters, highlights and lowlights of my life, I have already given examples of how I became controversial, and how my opinions conflicted with those of other people and degraded friendly personal relationships, unintentionally. From the presently 113 alternatives which I have assembled, I have already explained some five, concerned with *neutron stars*, *black holes* and their *entropy*, *SN explosions*, and *cosmic rays*. In order to make myself better understood, I will now, at the end, attach the whereabouts of another ten of them, as additional manifestations of the Gold effect. Historically ordered, they will be concerned with the (1) quarter-daily cosmic *gamma-ray bursts*, (2) feasibility of efficient '*in-situ*' *acceleration* (to high particle energies), (3) *astrophysical jet* production, (4) terrestrial *plate tectonics*, (5) the 1908 *Tunguska* catastrophe, (6) *hearts of the* (higher) *plants*, (7) our *atmospheric oxygen*, (8) electric

charging of our atmosphere, (9) the *law of entropy* in continuum thermodynamics, and (10) the 29 May 2006 outburst of the Earth's most voluminous recorded *mud volcano* near Sidoarjo, 30 km south of Surabaya.

(1) The first (extraterrestrial) *gamma-ray burst* was recorded on 2 July 1967, by an American military satellite. The first conference on (by then declassified) gamma-ray bursts was the Xmas 1974 Texas Symposium on Relativistic Astrophysics in Dallas, where Malvin Ruderman reported on more models than detected bursts. During the 1980s, the models for their sources homed in on Galactic neutron stars, but shifted (from Galactic) towards cosmic distances a decade later, when their arrival directions became isotropic, and when relativistic redshifts were discovered. Such a huge shift in distance would imply a shift of the involved energies by a factor of order 10^{16} , whilst spectral hardness and temporal sharpness still asked for (the same type of) neutron-star (or BH) sources. At this time, Hsiang-Kuang Chang and I judged the energetics more important than the statistics, and showed how an isotropy of arrivals can also be mimicked by a nearby (Galactic-disk) source population, at distances between 0.01 and 0.5 kpc. Relativistic redshifts are familiar from SS 433 since 1978, they need not imply large distances.

(2) During the late 1970s, an international consensus revived Enrico Fermi's transient idea that high-energy particles could be generated (in the Universe) in many repeated elastic collisions with fast-moving plasmas (shock waves), like pingpong balls reflected from approaching rackets [A. Bell, R. Blandford, G. Krinsky, 1977]. This mechanism of *in-situ particle acceleration* was hoped to explain not only the origin of the (highest-energy) cosmic rays, which were (thought to be) predominantly hadronic, but also of the extremely relativistic electrons which radiate in the astrophysical jet sources, i.e. explain energy transfers towards either hadronic, or leptonic particles. Whilst such selective mechanisms were proven to be feasible in the test-particle limit in which elasticity of collisions is a good approximation, and the law of (growth of) entropy is not violated, I could not believe that it would still hold true for the high transfer efficiencies required to explain the observations. It has been my conviction from the very beginning that the celestial hard-spectrum sources derive their high particle energies from the reconnection of strained magnetic fields.

(3) If in-situ electron boosting in the *jets* of the *bipolar astrophysical sources* conflicts with the second law of thermodynamics, I could only see the braking of a magnetized rotator as a feasible source of abundant (extremely relativistic electron-positron) pair plasma, whose buoyant escape from the deep (axi-symmetric) potential well of its central engine leads to never-splitting, self-focussing (due to minute inertia) antipodal twin-jets with all the observed properties, which my co-author Gopal Krishna made sure were not ignored by us. This uniform explanation of the various jet sources profited from my strong disbelief in any black-hole engine, which would lack enough time-variable magnetic flux. As mentioned above, our conviction was shared in 1981 by Phil Morrison, at Socorro, also in his talk at Albuquerque by Martin Rees, but not documented, and did not reach the scientific community. Since 2004, Gopal and I are convinced that the pairs in the jets perform stable, loss-poor, mono-energetic $E \times B$ - drifts in self-convected electromagnetic fields, with bulk Lorentz factors of $10^{3 \pm 1}$. The observed broad power-law spectra of their radiation are formed in situ, on collision with (heavy) ambient obstacles, in direct electric discharges (through their convected equi-pressure voltages).

(4) The crust of Earth consists of more than a dozen light, thick continental plates pushed around by heavier, thinner oceanic *plates* which move apart from active, quasi-central 'mid-ocean ridges', at average speeds of order cm per year, in discrete, roughly half-centennial jerks in the form of opposing strips, bordered by (water-tight, non-volcanic) 'transform faults'. What keeps them moving? When Alfred Wegener recognised the *continental drift* of South America away from Africa, from their shapes, geologies, and biotopes, Harold Jeffreys was unconvinced because no large enough forces were known in the crust that could propel their motion. Both of them were right, I think: Wegener was proven right by the detection of a striped magnetic polarization of the ocean floor, decades after his death—and nowadays by direct velocity measurements between anchored stations—and Jeffreys was right because the necessary pressures must be comparable with the weight per area of a jerking strip when tilted vertically, orders of magnitude larger than under stationary surface conditions. Such enormous pressures can be brought up from the molten core of Earth, I think, when hot magma, rich in natural gas, forces its way up to the surface, buoyantly, via overhead stoping, convectively heated from below. Such light vertical overpressure tubes owe their existence to an instability of a hot melt underneath its solid; Axel Jessner and I have called arrays of them a 'volcanic fence', in 1986. We are still convinced that the (alternative) mainstream explanation, convection rolls in the (solid!) mantle, is dynamically impossible.

(5) On 30 June 1908, early in the morning, hell broke loose in the Siberian taiga, near the river Stoney *Tunguska*: Trees were felled in an area comparable in size with the Saarland, noises roared, flames were seen in the sky, reindeer were partially burnt, and the few hunters and herdsman who slept in this area were blown out of their tents. For more than 50 years, native scientists believed in the impact of a cosmic meteorite, asteroid or cometary nucleus, but no gram of it has ever been found. Guided by the Moscow geologist Andrei Ol'khovarov, the vast literature, and a personal visit of the site, I have convinced myself that the destruction did not come from above, rather from below: A giant outburst of natural gas, at the site of an ancient volcano, near the center of the Siberian craton—most likely a kimberlite—whose supersonically discharging gases converted the trees near the ≥ 5 outflow centers into 'telegraph poles' (like in Hiroshima), left other patches of trees (in the valleys) intact, and blew a zero-net-momentum (radial) treefall pattern at larger distances. Tiny snowflakes in the exosphere caused the succeeding 3.5 bright nights west from the blowout, as likewise reported for Krakatoa. In accordance with crater statistics, tectonic destructions are some 30 times more frequent than impact destructions at the same energy.

(6) Like animals, plants require circulating water for their maintenance and growth: for lifting the nourishing ground water to their crowns, and for redistributing the photosynthesis products of their leaves to all the (other) sites of growth: branches, buds, fruits, and roots. What propels this necessary circulation, to heights reaching 140 m in extreme cases? The literature talks of osmotic and capillary forces, transpiration, and coherence. But transpiration is a loss mechanism, for both animals and plants, which must be replenished by drinking; coherence helps keep long threads of water from tearing, but does not perform work. And moreover, plants are known to exert gigantic root and stem pressures, of order 10 bar, manifested by splitting, or bending solid obstacles, and by the exudation of pure water during guttation, and from wounded organs. This root pressure builds up in the roothair zone of young root tips where water from the ground is

sucked in osmotically, a pressure that is maintained in transit to the central cylinder, via a *reverse osmosis* which requires mechanical work. Work must therefore be performed so that the leaves in the crown can pull osmotically a second time. The necessary mechanical pumps are apparently located in the endodermis and pericycle, realized by submicroscopic desmotubuli, millions of them by number, pumping at frequencies comparable to the human heart. I like to call them the '*hearts*' of the plants.

(7) Present life on Earth depends on *atmospheric oxygen*, which is commonly thought to have been liberated by oceanic cyano bacterii, working for blue-green algae along narrow strips of low-latitude coastlines, as a generous byproduct of photosynthesis. When they die, they leave limestone and silicates behind. Where is the net liberated carbon? Maybe I am mistaken, but to me, the biospheric oxygen production is cyclic (on average), to a high percentage. Instead, oxygen is liberated abiogenically from the very beginning of Earth, by a splitting of water vapour via solar UV in the high atmosphere, and by subsequent buoyant escape of hydrogen from the exosphere, much more (when integrated over time) than is presently stored in the atmosphere.

(8) Thunderstorms are known to everybody, but how are the thunderclouds charged? And why is our *terrestrial atmosphere* everywhere permanently *charged*, with a daily periodicity, typically to 10^2 Volt/m near the ground? And why do airplane pilots occasionally see giant discharges way above the troposphere, all the way up to the ionosphere, in the form of vertical blood-red '*sprite*', superluminally expanding ionospheric rings called '*elves*', blue inclined jets, and gigantic jets? Apparently, transient charging in excess of kVolt/m happens even in the highly conducting thermosphere. Its unknown source frustrated the experts some 135 years ago, but was later '*explained*' by brute force, as the thunderclouds themselves, arguing that their visible discharges were overcompensated by charge motion in the opposite direction. Gernot Thuma and I were not convinced. We prefer to think that the upper atmosphere is permanently charged by transluminal (!) downward-moving electrons, knocked off by incoming cosmic rays from atmospheric gas molecules, with long mean-free paths. Near the upper edge of the troposphere, some 10 km high, residual free electrons accumulate on atmospheric aerosols and hitchhike with their heavy-enough subpopulation, all the way down to the ground, continuing the incoming charging current to the highly conducting (moist) surface of Earth.

(9) The law of (the permanent growth of) entropy tends to be proven for closed subsystems near thermal equilibrium, via box thermodynamics, as an inequality. Can it not be formulated continuously, by a transport equation, for arbitrary systems? Indeed this has already been done in Landau & Lifshitz V, but called shyly the '*heat equation*' by them: The continuum thermo-hydrodynamical equations can be routinely derived from a statistical description (via the hierarchy of kinetic equations for the *r*-particle distribution functions), as the conservation laws for the collision invariants: 4-momentum and charges. The macroscopic dynamics of a fluid system are obtained already from 3-momentum conservation, well-known as the Navier-Stokes equations, whilst energy conservation during collisions yields an independent, seemingly redundant description. Instead, the energy balance keeps track of locally randomized energy, and can be formulated as the *second law*, the continuous *growth of entropy* during mixing, dissipation, and chemical reactions, by degrading macroscopically ordered motion.

(10) The world's largest accident (GAU) during search for petroleum happened on 29 May 2006 on Java, some 30 km south of Surabaya: Instead of natural gas and/or petroleum from the borehole sprung forth a mud fountain from a nearby rice paddy (150 m), early in the next morning, after a sudden 'kick' at a depth of 2834 m, when a deep-lying limestone layer was pierced. The upsurge of mud increased from day to day, reaching a rate of $10^{5.3} \text{m}^3/\text{day}$ after several months. How to stop it? Experts conceive of years to decades of continuous supply, when compared with similar events elsewhere. Perhaps the ten mountains on Java owe their existence each to a similar outburst. The experts have tried, in vain, to stop the mud ejection with concrete balls chained in steel, dumped into the 'Big Hole', of diameter 50 m. My advice: I expect a dome-shaped underground reservoir, a few km across, filled from below with natural gas at near-liquid density, because of the high local pressure. This reservoir has probably been tapped in a low place, with the compressed-gas layer above it. Via acoustic sounding, a peak of the underground dome should be sensed, and tapped via new drilling, in order to reduce the high overpressure, and at the same time harvest petroleum, as originally planned. So far, my messages to the reporting journals have not been responded to.

Ups and downs on international stages

Scientific opinions can form in various ways: via communications between individuals—both orally and in written form: via (snail) mail, or nowadays via e-mail—via colloquium talks in the world's respected centers, and also during regular yearly, or biennial international conferences which bring influential scientists together at hospitable resort sites. One such meeting place is Italy's small, wild Vulcano island, half an hour's hydrofoil ride north of Sicily, where Frascati's Franco Giovannelli has succeeded in bringing together leading astrophysicists from all countries year after year, starting in 1984, to discuss the multifrequency behaviour of cosmic sources "seriously even if smiling". He first invited me in 1995, quite unexpectedly, and nine further times since. I was permitted to talk freely, several times per workshop. Franco helped me gain attention, by mentioning my work, even elected me as one of the three concluding speakers, and found supporting referees for their printed versions in the proceedings.

Could I convince my contemporaries, of a significant fraction of my (by now 113) alternatives? At most a very limited number of them: it is not easy to swim against the tide. Still, on 15 November 2000, I was honoured at the XXth meeting of the Astronomical Society of India by a one-hour felicitation in front of the complete assembly - in the name of the citizens of Gorakhpur - and overwhelmed with presents, jointly with Govind Swarup, and in the presence of my wife Ulrike. It was an incomparable highlight in my scientific life.

Acknowledgements: My warm thanks go to all those older and younger colleagues who have helped me gain insight into the physical functioning of the Universe, also of our threatened Earth. My self-confidence has been strengthened by quasi-regular invitations to Maribor (Slovenia) by Marko Robnik, to India's warmth, to Linz by Eckhard Krotscheck, to Taiwan by Hsiang-Kuang Chang, and to Turkey by Mehmet Oezel. Klaus Hasselmann's friendship and his metron approach to the yet unsolved elementary-particle

problem have formed a life-long tie. In recent years, my weekly seminar members Hans Baumann and Gernot Thuma keep me excited (and contradicted), Christoph Hillemanns and Udo Wernick give me independent support, Günter Lay reliably helps me with the electronic data handling, and my son-in-law has improved the English of this writeup. To all of them go my thanks.

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My Struggle with Ginsparg (arXiv.org) and the Road to Cyberia:

A Scientific-Gulag in Cyberspace

by C. Castro Perelman

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Abstract: I describe in the following pages the sequence of events in the past years that led to being blacklisted from the open electronic archives (<http://arXiv.org>) administered by Paul Ginsparg, now at Cornell University (Ithaca, New York) and formerly at the Los Alamos Labs (New Mexico). The arXiv.org electronic archives is the CNN and Fox News of Physics, Mathematics and other disciplines. They have bent over backwards in trying to deny that there is such a thing as a blacklist. In fact they [*arXiv.org*] accuse some of us of improperly using the name “blacklist”. This is preposterous because when the arXiv.org denies that there is such a thing as a blacklist it amounts to saying that a tight-closed-hand should not be called a fist. Whether it is a black, white, yellow...or a phantom-list, it does not change the true facts described below over the past years where I depict the level of arrogance, corruption, smugness, wiles from the arXiv.org and, in particular, the psychological violence inflicted by those in positions of power (Ginsparg) on the general citizen, like myself. Even more tragically is that most of the members of the physics establishment that I know of, and who have been informed of the illicit censorship and blacklisting of scientists, have been in many ways complicit with Ginsparg's shameful practices because they have remained silent and allowed this brutal suppression of scientists to happen for so long, all the way back to the early days (1990's) when the electronic archives were ran by Los Alamos. Actually, there are many scientists who find this blacklisting quite amusing and “necessary” for the “progress” of science. Unfortunately, among these I must include old “friends” of mine who don't want to associate themselves with scientific “lepers” who have been condemned to Cyberia: a Scientific Gulag in Cyberspace created by the “commissars” of the scientific establishment.

The troubles began with the Nuclear Physics B journal.

Lantz Miller (New York resident, Columbia University Journalist student) in his article in *Crosscurrents* magazine:

“Migrations of Ideas in the Age of the Internet and the Socio/Cultural Hegemony of the Scientific Community: Case Studies”

that was based on his talk at a Rutgers University Conference in March 2002 (New Jersey) sponsored by the Sociology Dept, described very accurately the origins of my problems with Ginsparg.

In the late Fall of 1999 I received two positive referee reports of a mainstream paper submitted to Nuclear Physics B (NPB). The editorial office (Elsevier publishers in Holland) then asked me for a disk file (Tex file) of such paper, another intended sign of publication. A few months later in January 28, 2000, I received a highly suspicious declination letter (e-mail) from NPB (Paul Schuudenboom) after a member of the Editorial board had shown a particular interest in my paper but later vetoed its publication (I think it may have been H. Ooguri, now at Caltech, but I have no way of proving this due to the secrecy of journal).

I would have not minded at all if they have asked me to revise the paper to fit the needs of the member of the Editorial board. After receiving this highly suspicious declination letter, the co-director of my center in Atlanta, CTSPS (Center for Theoretical Studies of Physical Systems in Clark Atlanta University) Prof. Carlos Handy wrote a letter to NPB expressing our concerns for this declination letter, especially after having received two positive referee reports and having sent them the disk file of my paper.

What we received back in response from NPB was yet another slap in the face. Not only they insisted that they had done nothing wrong, because they had not “promised” anything, but they had looked at my recent work in the e-archives, ran at the time by the Los Alamos Laboratory in New Mexico, and found my work to be outlandish. NPB was clearly referring to my work on the Extended (“new”) Relativity Theory (later developed in Clifford Spaces) based on earlier work by Laurent Nottale (in 1983) on the Scale Relativity Theory. In the latter theory the Planck length scale was postulated as a minimum scale (resolution) in Nature, not unlikely the speed of light is postulated as the maximum attainable speed in the ordinary Relativity Theory.

However, what NPB seemed to have forgotten entirely is that my NPB paper that had received two positive referee reviews was a mainstream paper that had nothing to do whatsoever with my work on the Extended (“new”) Relativity theory. In fact, an extended version of my original NPB paper has been published in the first rate journal Classical and Quantum Gravity 20 (2003) 3577-3592 : “Anti de Sitter Spaces (AdS_{2n}) from $SO(2n-1, 2)$ Instantons”.

The reader may ask, what does all this have to do with Ginsparg? Well, lo and behold, when I tried submitting my most recent paper (at the time) in early February 2000 to the hep-th (high energy physics theory) category my paper was removed and displaced to the general physics category (the bottom of the pile in readership and audience). It does not take Sherlock Holmes to figure out that it was NPB who complained to Ginsparg that Castro was posting outlandish papers in the Los Alamos archives. After all NPB admitted in writing that they had looked at my work in the Los Alamos archives in late January 2000 and found my work outlandish. The reader may say that this is nothing but circumstantial evidence and that it does not constitute a proof. People have been arrested, tried, convicted and executed in the USA based solely in circumstantial evidence.

Having explained the sequence of events that led to the beginning of my problems with Ginsparg in early February 2000, I must add that I wrote a letter to NPB denouncing what I thought to be their shameful behavior. They politely wrote back and told me that they will inquire about this unfortunate incident. I never heard a word from them again (to be expected).

The next time I tried posting a paper to the hep-th archive-category the same thing occurred, it was removed automatically and diverted to the general physics category without the possibility of cross-listing to other categories. I realized that the robot at the archives was programmed to recognize my e-mail address and to divert my papers automatically from the hep-th category to the general physics category.

What to do next ? Well, I decided to post my next paper from the e-mail account of one of my co-authors of a joint-paper, Jorge Mahecha, directly from Trieste, Italy, since I was visiting Trieste at the time. It worked , the paper was successfully posted in the hep-th archive-category in August, 2000, without any problems until, until

Until I was travelling in France and Spain in August, September, when Jorge Mahecha e-mailed me telling me that Ginsparg had written an ultimatum giving us a two-day period to remove the fraudulent Cambridge affiliation from one of our co-authors, M.S. El Naschie, chief Editor of the Journal of Chaos, Solitons and Fractals. Apparently Ginsparg received a complaint from Cambridge University (later I found out that it was Michael Green). Since Jorge Mahecha had only two days, he had no choice but to comply and removed the El Naschie's Cambridge affiliation without been able to make a thorough inquiry as to the veracity of the horrible allegations against M.S El Naschie. Shortly after, Jorge Mahecha opted to remove the paper altogether to avoid any further problems, before we had an opportunity to perform an investigation of the veracity of the serious charges raised by Michael Green (at Cambridge University) against M. S. El Naschie.

Having described the chain events in late January 2000 (the NPB fiasco), the subsequent removal of my papers to the hep-th category in early February 2000, and afterwards, the El Naschie-Michael Green incident in late August 2000 , which I will analyze in further detail shortly, I will now reveal a personal e-mail written by Ginsparg himself to Fred Cooper at Los Alamos which is full of insults, lies and threats.

The insults, multilayered lies and threats from Ginsparg

The Actual e-mail from Ginsparg to Fred Cooper at Los Alamos:

Date: Fri, 29 Sep 2000 13:47:02 -0600 (MDT)

From: Paul Ginsparg ginsparg@lanl.gov

To: fcooper@physics.bc.edu

CC: simeon@mmm.lanl.gov, schwander@horse.lanl.gov

Subject: Re: Castro Clarification

a) castro is an obvious nut and all of his papers are abject nonsense b) castro had a paper with a co-author whose affiliation was forged as DAMTP (Cambridge), and we received a complaint directly from DAMTP. he was given a deadline to correct or remove the affiliation, or lose submission privileges. he CHOSE not to comply.

conclusion: castro is more than welcome to publish in conventional journals we don't have time for him here, and he is fortunate that he is permitted in the General Physics category.

(next is a phrase from F. Cooper's initial letter to Ginsparg)

“Carlos Handy at Clark Atlanta was a former post-doc of mine and this person's boss. If you could handle this with some sensitivity, I would appreciate that”

(Ginsparg continues)

note that if clark university will insist on this, then we will cease to regard clark university as a responsible accredited institution. (we rely on institutional affiliation for effective endorsement. note most of his recent activity has been from Italy so we have no idea why clark univ is even involved at this point.)

you should explain to your former postdoc Carlos Handy that he is the one who should be embarrassed at this point. we wouldn't care except this one idiot has wasted more time than the average ten idiots... i hope this is sensitive enough

pg.

The analysis of the multilayered lies and threats from Ginsparg

Ginsparg began to remove my papers in the hep-th archive-category in February 2000, 6 months prior to the El Naschie-Michael Green incident. Ginsparg concealed this fact from Fred Cooper by making me look as the "bad guy". The El Naschie-Michael Green incident was nothing but diversion from the truth. Truth that Ginsparg did not wish to reveal to his boss, Fred Cooper.

In addition, Ginsparg accused me of having posted a paper with a false institutional affiliation by one of my colleagues, M. S. El Naschie who is the chief editor of the Journal of Chaos, Solitons, and Fractals (CSF) published by Elsevier. However, the truth of the matter is that M. S. El Naschie had listed his affiliation, for many years, and at many public conferences, etc....., as DAMTP - Cambridge. I had no knowledge whatsoever that M. S. El Naschie was not permitted to use the Cambridge affiliation. How could I have known this before hand, when the affiliation of M. S. El Naschie appearing in the CSF journal, and in many conferences ... was listed as DAMTP-Cambridge for many years? Why am I to blame?

Frank (Tony) Smith, a lawyer, scientist and another blacklisted wisely asked : Why wasn't Elsevier Science Publishers in Holland attacked by Cambridge University? Why weren't the organisers of the many public conferences where M.S El Naschie attended not attacked?When during all these years M. S. El Naschie was using the Cambridge affiliation?

Jorge Mahecha (from the ICTP in Trieste, Italy) removed the false institutional affiliation of M. S. El Naschie from our paper submitted to the e-archive within the two day deadline demanded by Ginsparg. Shortly after, within a period of 8 days Jorge Mahecha removed the paper altogether. Since I was traveling in France and Spain during that time, Ginsparg lied when he accuse me of “choosing not to comply” after his ultimatum. Jorge Mahecha who was in Trieste, Italy to receive Ginsparg's e-mail-ultimatum did comply.

Alex Granik and Jorge Mahecha were among the co-authors of two papers with M. S. El Naschie and myself. Nevertheless, Alex Granik (University of the Pacific, Stockton, California) and Jorge Mahecha (University of Antioquia, Medellin, Colombia) have not been blacklisted by Ginsparg. They are permitted to post their papers to any category of the arXiv.org without any problems.

Why did M. S. El Naschie illicitly use the DAMTP-Cambridge affiliation for many years? It seems that because he was a friend of the former Head of the Physics Dept in Cambridge University that he took certain liberties for many years. After the El Naschie-Michael Green incident took place, he is no longer using the DAMTP-Cambridge affiliation.

Let us proceed now with the threats made by Ginsparg to ban my center CTSPS in Atlanta from submission privileges if they defended me in my right to post papers in the hep-th archive category. Next we will display the letter from Prof. Carlos Handy (CTSPS) expressing his disgust after reading Ginsparg's lies, insults and threats.

The response of the CTSPS to Ginsparg's insults, offenses and threats

This message below was written by Prof. Carlos Handy in 2000 (my boss in Atlanta) to Prof. Geoffrey West at Los Alamos Labs in response to Ginsparg's extremely offensive e-mail. Fred Cooper was the former boss of Paul Ginsparg at Los Alamos before he came to Cornell. At that moment Fred Cooper was visiting Boston College, thus Geoffrey West was left in charge back in Los Alamos, the T-8 Division. This explains why Prof. Carlos Handy is responding to Prof. Geoffrey West.

Please notice that my center (CTSPS) in Atlanta interpreted very seriously Ginsparg's threats. The threats that to any English-speaking native [as pointed out by Jack Sarfatti] unambiguously implied that Ginsparg meant to ban the whole CTSPS from submission privileges if they insisted in supporting me. This dictatorial behaviour from Ginsparg in threatening to ban a whole research center from submission "privileges" is despicable and cannot be tolerated in a democracy.

On the other hand, nobody denies Ginsparg's right to have an opinion by uttering the words "Castro is an obvious nut". But the mere fact that Ginsparg is entitled to an "opinion", it however does not entitle him to remove papers from scientists he does not like! This is a point that unfortunately so many of my so-called "friends" have been unable to understand! I have numerous publications in peer reviewed journals, I honestly doubt that Ginsparg read all of my journal publications that would have allowed him to write objectively that "all of Castro's papers are nonsense".

E-mail from Dr. Carlos Handy to Geoffrey West at Los Alamos :

Date: Tue, 3 Oct 2000 13:42:41 -0400

From: "Carlos R. Handy" handyman@ctsps.cau.edu

Message-Id: 200010031742.NAA50390@ctsps.cau.edu

To: gbw@lanl.gov

Subject: Important

Cc: handyman

Status: R

Hi Geoffrey, sorry that things did not work out w.r.t. NSF proposal. I have been in contact with Fred Cooper who indicated that you are the boss, since he is away at Boston College.

There is a controversial (anti-string) scientist [inaccurate description of myself] who we have supported for some time as a “permanent visiting scientist”. He is very colorful, knowledgeable, and (I believe) smart. His name is Carlos Castro. He and his colleagues have been having a hard time with Ginzparg vis a vis getting their articles on the HEP LANL archives.

Ginzparg said to Fred that Castro is a “Nut”. In addition, he accused him of having posted a paper with a false institutional affiliation by one of his colleagues (M. Al Naschie) who is the chief editor of the Journal of Chaos, Solitons, and Fractals (published by Elsevier). In any case, this guy (Naschie) has listed his affiliation, for many years (and at many public conferences), as DAMTP - Cambridge. Castro had no knowledge of the legitimacy of this, and he and his colleagues removed the institutional affiliation from their submitted archive paper within the two day deadline demanded by Ginzparg.

I recognize that Castro is controversial, and many people don't follow his papers because of all the intricacies of his arguments. However, he has colleagues, with whom I have talked, who are very sober and conventional. I believe that much of his work stimulates the mind and is an honest effort with no obvious technical fallacies. Of course, if a well documented technical declination of his work is forthcoming, by a group of unbiased physicists, then there is nothing more to say.

However, it does appear that the people that are giving him a hard time are those that feel threatened by his anti-string posture. I have indicated to him that in my opinion, the LANL archives are Federally funded, and he, Castro, through our institutional affiliation, has the right to publish his articles there.

Ginzparg told Cooper that if my school continues to back Castro up, we will be black listed from accessing the archives. In addition, he indicated that I should be embarrassed for supporting Castro. In the absence of any cogent, well documented, rebuttal of his papers, I cannot subscribe to Ginzparg's perspective. In fact, he has opened himself up for legal charges on many fronts. By coincidence, one of the guys conversant with what is going on is both a lawyer and a physicist (Tony Smith). Castro is a U.S. citizen, and in the absence of any proof of the sort I have indicated, deserves to have his papers respected in the manner indicated by the archives guidelines.

The situation is becoming nastier. I am appealing, on a personal level, that in light of all the above, that you or Fred, allow Castro and his colleagues to publish their works in the archives. I do not appreciate Ginzparg's presumptuous attitude. Castro is prepared to take this matter to all and any higher authority, including DoE, and minority members of Congress.

Again, without any hard evidence to the contrary, as outlined above, I can only support him (Castro) in this.

Sincerely,

Carlos Handy

I am fully blacklisted since the beginnings of 2003.

How I avoided Ginsparg's radar for a period of Time

In the two year period of 2001-2002, during the transition from Los Alamos to Cornell of the e-archives, I was able to avoid Ginsparg's radar and post 20 papers in the hep-th archive category, both as a single author and with other co-authors, from different e-mail accounts of colleagues in Colombia, Italy and Slovenia. 11 of those papers have been published in peer review journals and 2 others have been revised and are currently under peer review [I don't always update the journal references of my past papers posted in the arXiv.org] .

But starting in 2003 until the present day I have been fully blacklisted by the arXiv.org. I tried posting two papers at the beginning of 2003 from the Institute of Mathematical Sciences in Chennai (Madras, India) but they were removed immediately prior to appearing in the daily listings. These 2 papers have been published in peer review journals:

(i) "On Non-extensive Statistics, Chaos and Fractal Strings" published in *Physica A* 347 (2005) 164-204; <http://www.sciencedirect.com/science> , available online 15 September 2004 and (ii) "AdS_{2n} spaces from $SO(2n-1,2)$ Instantons" published in *Class. Quant. Gravity* 20 no.16 (2003) 3577-3592

The arXiv.org did not accuse me this time of being an obvious nut like Ginsparg did before but of misrepresenting my affiliation with the CTSPS center in Clark Atlanta University, while before Ginsparg himself had threatened to ban the whole research center CTSPS in Atlanta from submission "privileges" if they [CTSPS] insisted in supporting me. Below I enclosed the letter, sent in two different occasions in 2003, to the arXiv.org by Prof. Carlos Handy (co-director) corroborating my affiliation and association with CTSPS.

Despite that Prof. Carlos Handy sent his first letter to the *arXiv.org* in mid March 2003, I tried posting a paper from Jorge Mahecha's account in Colombia during the summer of 2003, (he was a co-author of this paper) but the paper was removed as well. Notice that Jorge Mahecha has no problems in submitting papers to the archives on his own. This paper was removed because it had me as a co-author. The paper I wrote with Jorge Mahecha:

"Fractal SUSY QM, Geometric Probability and the Riemann Hypothesis" was published in the *Int. Jour. Geometric Methods in Modern Physics* 1, no. 6 (2004) 751-793.

In view of the removal of this last paper, Prof. Carlos Handy resent in July 1, 2003 the same letter (below) to the *arXiv.org* corroborating my affiliation and association with the CTSPS in Atlanta. The letter sent by the co-director of CTSPS (in two occasions) should have been satisfactory. This fact, in addition to the earlier offensive letter from Ginsparg threatening to ban the whole CTSPS center from submission privileges for supporting me, should have been enough for them. Clearly nothing is enough for these individuals. They set arbitrarily rule number N , when you obey it, they cook up rule number $N + 1$, and so forth ad infinitum.

After this second letter from Prof. Carlos Handy in July 1, 2003, I tried submitting another paper in early January 2004 that was co-authored with Matej Pavsic (Ljubljana, Slovenia) and it was removed once again

under the very same pretext !!! I did not even had the chance to move from rule N to rule N + 1 This last paper was a commissioned review article to the IJMPA journal at the request of D.V. Ahluwalia-Khalilova (one of the editors in Mexico) and was accepted for publication. When Matej Pavsic protested the removal of our paper (he can also post papers on his own without any problems) the arXiv.org then came up with a blackmail offer described next!

The trappings behind the blackmail offer from the arXiv.org

Yes, I just wrote the words blackmail this time in contradistinction to blacklisting. I have just described above the shifting rationale for being blacklisted: from being an obvious "nut"; from posting papers without the "permission" of my co-authors. This was M.S. El Naschie's great excuse for having used illicitly the DAMTP-Cambridge affiliation for many years and in many conferences !!!.. excuse that he must have given Ginsparg after the El Naschie-Michael Green incident discussed earlier. Then I was accused of misrepresenting my affiliation at the CTSPS center in Atlanta....after Ginsparg had threatened to ban the whole center CTSPS in Atlanta if they insisted in supporting me..... After all these experiences what is next ??? It is time to reveal to the readers the trappings behind the wonderful "blackmail offer" from the moderators at the arXiv.org. I describe best this blackmail offer by attaching the e-mail correspondence between Frank (Tony) Smith (another blacklistee) and Matej Pavsic.

Date: Sat, 10 Jan 2004 00:42:29 -0500

From: Tony Smith f130smith@mindspring.com

Subject: correction: arXiv and Carlos's affiliation

Cc: f130smith@mindspring.com

Carlos, and Matej, it is my opinion (as a lawyer) that the statement from "moderation for arXiv.org" that "... If ...[Carlos]... has no institutional affiliation, that is also fine. Just remove it and resubmit. ..." is a trap to try to trick Carlos into admitting to a lie, that is that he is not affiliated with Clark Atlanta. Matej, I have sent you a copy of an e-mail message from Carlos Handy to "moderation for arXiv.org" moderation@arXiv.org that clearly stated that Carlos Castro IS affiliated with Clark Atlanta.

Below my signature is a copy of that message with headers included, so that you can see that it was indeed on 1 July 2003 sent to "moderation for arXiv.org" moderation@arXiv.org.

It is clear to me that "moderation for arXiv.org" is NOT telling the truth when it says "... the given affiliation cannot be verified. ..." if you take any reasonable interpretation of the English words used.

Of course, Matej, you have my permission to send this information directly to "moderation for arXiv.org", although in my opinion "moderation for arXiv.org" is already well aware of it and the facts stated in it, and is now just trying to trick Carlos Castro into admitting to a lie.

Tony

Letters from the co-director of CTSPS in Atlanta to the arXiv.org

Below is the letter written by Prof. Carlos Handy to the moderators (the cyber-police) of the Los Alamos-Cornell archives (arXiv.org). (He sent the first copy in mid March 2003)

One of the latest reasons for being attacked by the cyber-police (a.k.a the “moderators”) is because I don't use my e-mail address: castro@ctsps.cau.edu in Atlanta. This is a vile accusation because they are hiding the fact that my Atlanta, Trieste (Italy) e-mail addresses have been targeted in such a way that the robot automatically removes and blocks any papers originating from those e-mails addresses. Another reason why I may have been attacked is for having applied for an NSF grant as an individual. I am an affiliate of the CTSPS center and since the center in Atlanta, as a whole, is already funded by the NSF I was advised that it would be better if I applied as an individual. Another reason for being attacked is that my name does not appear in the CTSPS website. The affiliates names of the CTSPS don't appear in the website, only the permanent members do. Please note that there are many scientists who don't have an academic affiliation and are allowed to post their papers from their company address and some from their home address!

Tue, 1 Jul 2003 08:42:01 -0400

Sender: handyman@hyper.cau.edu

Date: Tue, 01 Jul 2003 08:42:01 -0400

From: Carlos R. Handy handyman@ctsps.cau.edu

To: moderation for arXiv.org moderation@arXiv.org

Carlos Castro czarlosromanov@yahoo.com,

handyman@ctsps.cau.edu

Subject: Re: Carlos Castro

To Whom It May Concern:

My name is Dr. Carlos R. Handy, Co-Director of the Center for Theoretical Studies of Physical Systems, and well known to Dr. Fred Cooper, formerly at LANL T-8 (where I served as post-doc, 1978 - 81), and now at NSF.

Dr. Carlos Castro, an American citizen, is an affiliate of our Center, and has been one for at least six years. He has published papers with the name of our center, and university (Clark Atlanta University, not “Atlantic”). We recognize that he is controversial, but in those matters of particular concern and interest to us, he has always been very insightful and credible. We do not like the impression of an intellectual mafia that seems to be engaged in scientific censorship. More specifically, in the past, Dr. Ginsparg has made e-mail remarks to Dr. Fred Cooper, concerning our support of Dr. Castro, that we have regarded as prejudiced and offensive. This should be particularly troubling to your organization, which continues to enjoy government support.

I would be glad to submit a written letter, but I trust, with the particular reference to Dr. Cooper, that this might not be necessary. In any case, please send me your specific address, and I will mail such a letter immediately.

I understand that your archives should not be the repository for any irresponsible, purportedly scientific articles, however, we (CTSPS) do not support everyone, and until such time that Dr. Castro behaves in a manner questionable to us, we will continue to support him.

Sincerely,

Dr. Carlos R. Handy

Associate Professor of Physics

Co-Director, CTSPS

Conclusions: a Catch-22 situation when CERN (Geneve, Switzerland) terminates their external preprint server for blacklisted scientists

CERN Particle Physics Lab in Geneve, Switzerland announced in October 2004 that they were suspending their CDS-CERN-EXT-preprint service for members of other institutes. CDS = CERN Document Server. EXT = External. What is my honest interpretation of this message?

That no longer those blacklisted individuals by the arXiv.org will have a loophole where to post their papers. CERN, like other institutions worldwide is a mirror site of the arXiv.org at Cornell.

CERN in a very polite manner wrote:

“Authors from other institutes should be posting their papers from now on in the arXiv.org”

while at the same time one of the most famous lines originating from the arXiv.org are:

“We are not censoring you because you can post your papers in other outlets”

“We are not censoring you because you can submit your papers to peer review journals”

CERN is not the only institution that has shut down their CDS-CERN-EXT preprint service but Elsevier Science has also shut down their Mathematics preprint server. The arXiv.org holds an ever growing monopoly of what is posted (and what will be allowed to be posted) in cyberspace.

In order to thwart any further lawsuits the arXiv.org in January 2004 erected the facade of the so-called endorsement system (of authors by other authors) which is nothing but a smoke screen behind which they can reinforce more efficiently their censorship and blacklisting practices. One of the reasons for doing so is to prevent preprints from blacklisted scientists using a pseudonym. Many scientists are afraid of endorsing those who are blacklisted because they fear retaliations from Ginsparg and running the risk of being blacklisted themselves or having their endorser status revoked. See the note in the <www.ArchiveFreedom.org> home page.

According to Aaron Bergmann (when he was a student at Princeton) he wrote in a physics website that: “people read Castro's articles for fun.... “. Too bad that people at Princeton (and elsewhere) are now deprived of “having such fun” with my articles. Today, they have to go to the journals in order to have their fun and thank Ginsparg (arXiv.org) for that.

To finalize let us write the original quote by S. Coleman (Harvard): “*The problem with a global village is all the village idiots*” and which was later on echoed to the world by P. Ginsparg. This arrogant mentality is indeed consistent with the censorship and blacklisting of scientists reminiscent of the McCarthy era and that is going on at the e-prints archives arXiv.org ran P. Ginsparg at Cornell University.

The dire consequences of being blacklisted by the arXiv.org at Cornell

To illustrate, next I present excerpts from the most recent e-mail from A. Garret Lisi (November, 2007) whose work received a lot of attention lately in an article by the Daily Telegraph.

A Garrett Lisi <garrett.lisi@gmail.com> wrote:

Hello Carlos,

Related to your recent paper on E_8 Grand Unification: “A Chern-Simons E_8 Gauge theory of Gravity in $D=15$, Grand Unification and Generalized Gravity in Clifford Spaces” to appear in the Int. Jour. Geom. Methods in Mod. Physics vol. 4, no. 8 (December 2007); Do you have an electronic copy I could look at? I'm familiar with some of your work from reading “On Generalized Yang-Mills Theories and Extensions of the Standard Model in Clifford (Tensorial) Spaces.” [paper published in Annals of Physics]. There aren't that many of us playing with this stuff.

I hope I didn't unfairly preempt discoveries that overlap with yours. I would feel bad if you had come up with many of the same ideas, earlier than I did, but were prevented from posting them to the arxiv. If it makes any difference, I don't have possessive thoughts towards this “ E_8 theory” that is coming together—I think it may be nature's theory, and it's much bigger than me.

I understand that you've had trouble posting to the arxiv, which I sympathize with. However, I believe they've revised their policy a bit. Now, when papers come in that the conservative individuals don't like, they recategorize the paper to the general physics arxiv. This may seem like a slap in the face, and in a way it is, but it's not a very public one, as the arxiv has changed its numbering system so the classification is no longer prominent.

(Carlos wrote): I will send you a reprint of this paper when it comes out to your address in Nevada.

Please don't send me dead tree, it would just end up as kindling.

(Carlos wrote): Stephen Adler gives a lot of references about E_8 Grand Unification in his papers that you can get from the web. References that will also be helpful to you and that you did not mention.

Ah, you are right. Thank you for the references, I wasn't aware of these. I had a look at them, and fortunately for me they are largely speculative and there is not a great deal of overlap. Nevertheless, his 2002

paper is a very good review. If/when I have built up sufficient motivation to revise the paper I just posted, I will include his review.

(Carlos wrote): I know Tony Smith quite well. I like his unified model based on the Clifford (8) group.

Yes, Tony got a lot of things right. And I've had many good email exchanges with him. It's a shame he's had to live as such an outsider.

Best,
Garrett

I sent Garret Lisi a pdf file of of my E_8 paper (to appear soon in the IJGMMP) and other paper on "Octonionic Gravity, Modified Dispersion Relations and Grand Unification" that was recently published in the Journal of Mathematical Physics.

We all remember what happened in Nazi Germany. Life began gradually to become more and more difficult for the Jews, Gypsies, Communists, Homosexuals, Humansexuals,... and we all know the outcome. If journals decide to follow the same guidelines and prerequisites of Advances in Mathematics and Theoretical Physics, for example, that all authors must post their papers, firstly, in the arXiv.org, before submitting the paper to the journal, and failure to do so disqualifies the paper automatically, this will be the end to having any chances of publishing in any journals. This is irrefutably McCarthyism on a global scale as far as science is concerned.

Some may know the story in Holland (perhaps is fictional) of a skeptical non-jewish tenant in a building whose tenants were mostly jewish. Gradually, from month to month, the jewish families from the first, second, third... floor began to disappear.

The milkman began to ask the non-jewish tenant: "Have you noticed that the jewish family so and so of the first floor left so suddenly without any notice?". The non-jewish tenant responds: "Yes, I've noticed that , so what? I don't find this unusual".

The next month, the milkman asks the same question but now referring to the jewish family of the second floor. The non-jewish tenant responds: "Yes, I've noticed that, so what? I don't find this unusual"; and so forth and so forth until all jewish families from the building were gone.

I may be exaggerating a little bit here, but this is what is happening when many individuals ask: "What is the big deal of being blacklisted from the e-archives arXiv.org when the blacklisted scientists can post their papers in the internet and there are the journals?"

The arXiv.org is the combination of the BBC-CNN-Fox-Univision-AlJazeera News of Physics, Mathematics and other disciplines. Being denied access to this megalithic science news organization is like trying to preach one's ideas in the middle of the desert in Mongolia and hoping that a migratory bird will carry one's message to the nearest tent. We are being denied access gradually to the first floor, second floor, third floor... until we are out in the streets, and I mean it literally: without jobs, without money, without

dignity... In my opinion, the only problem with a global village is all the village commissars who agree with the Coleman-Ginsparg's arrogant policies towards the “village idiots” ...

To finalize this conclusion, I will display a recent letter by a concerned individual. Raphie Frank: <raphiefrank@earthlink.net> recently wrote:

Dear [Friend],

I quite vigorously disagree with your assessment regarding the inconsequentiality of being “blacklisted” by ArXiv.org. Dr. Carlos Castro Perelman's career has been rather severely compromised, and I have a fair amount of background information to support that, but am at this point obliged to maintain confidences. Some Consequences of Being “Blacklisted” can be found in <http://www.archivefreedom.org/freedom/consequences.html>

The blacklisting at Cornell, incidentally, appears to extend to the e-archives from the Abdus Salam Int. Center for Theoretical Physics (ICTP) in Trieste, Italy. Imagine you had an online business and Google and Yahoo and MSN refused to list your web page. You would be *invisible* and would be pretty quickly out of business, even if other search engines listed your web page. ArXiv and its mirror websites are the equivalent of Google, Yahoo and MSN combined when it comes to scientific knowledge and the marketplace of ideas.

Updates

The Abdus Salam International Center for Theoretical Physics in Trieste, Italy has recently launched an e-preprint archive and has joined also in the same censorship and blacklisting of scientists. This is what I call the translational-symmetry invariance property of blacklisting in a flat world of the mind.

For case histories of blacklisting and news stories, radio interviews,... see <<http://www.ArchiveFreedom.org>>.

Prof. Carlos Handy has left Atlanta and is currently the chairman of the Physics Dept at TSU (Texas Southern University) in Houston, Texas.

Lantz Miller is now a resident of France.

Many readers have written to their Congress and Senate Representatives.

The previous presidents of Cornell University have done nothing to stop this blacklisting.

R. M. Santilli at the Institute of Basic Research in Palm Harbor Florida has filed a multi-million dollar lawsuit against the president Cornell and other individuals. The prior lawsuits by Frank Tony Smith (Georgia) and Robert Gentry (Tennessee) were dismissed on jurisdiction grounds but not on merit.

Basic Cause of Current Corruption in American Science

by J. Marvin Herndon

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URL: <http://UnderstandEarth.com>

[June 2007]

The purpose of science is to determine the true nature of Earth and Universe. The intent is elegant in its simplicity and the goal is laudable, certainly one the highest aspirations of humankind. All this seems so readily attainable, but yet there is nearly overwhelming American institutional opposition. The purpose of this brief communication is to address the principal cause for the institutional and wide-spread perversion and corruption of science in America.

Worldwide, science is and always has been a strange activity, capable of providing the underpinnings for engineering advances that may benefit civilization on a grand scale, but an activity virtually without market value, at least in the important initial “blue sky” stages. Financial vulnerability is the Achilles heel of science. One may begin to understand what went wrong with American science by first looking at the way science worked and was supported before World War II.

Prior to World War II there was little government financial support for science. While supporting himself as a Swiss patent clerk, Albert Einstein explained Brownian motion, the photo-electric effect, and special relativity. Niels Bohr, supported by grants from the Carlsberg Brewery, made fundamental discoveries about atomic structure and served as a focal point and driving force for the collaborative effort that yielded quantum mechanics, the field of science underpinning solid-state electronic technology that so benefits the world today. Although money for science at the time was in short supply, scientists maintained a kind of self-discipline. A graduate student working on a Ph.D. degree was expected to make a new discovery to earn that degree, even if it meant starting over after years of work because someone else made the discovery first.

Self-discipline was also part of the scientific publication system. Prior to World War II, when a scientist wanted to publish a paper, the scientist would send it to the editor of a scholarly journal for publication and generally it would be published. A new, unpublished scientist was required to obtain the endorsement of a published scientist before submitting a manuscript. The concept of “peer review” did not yet exist.

World War II came and then suddenly there was serious and urgent need for scientific and technological advances deemed necessary for the global war effort. Government monetary support for science commenced.

On November 17, 1944, U. S. President Franklin D. Roosevelt wrote to Vannevar Bush, Director of the Office of Scientific Research and Development, asking for recommendations concerning post-World War II support for science, stating in part, “.... The information, the techniques, and the research experience developed by the Office of Scientific Research and Development and by the thousands of scientists in the universities and in private industry, should be used in the days of peace ahead for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living.... New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness, and drive with which we have waged this war we can create a fuller and more fruitful employment and a fuller and more fruitful life.”

On July 25, 1945, Vannevar Bush responded to U. S. President Harry S. Truman, transmitting the report *Science, the Endless Frontier*, which was to become the blueprint for peacetime U. S. Government support for science.

In 1951, the U. S. Congress established the National Science Foundation (NSF) to provide financial support for post-World War II scientific research. Soon thereafter, someone at NSF or on the National Science Board, which is charged with oversight of NSF, had an idea, a really corrosive idea, the implementation of which would lead to the perversion and corruption of American science for decades ahead. The idea was that reviewers of scientific proposals to NSF for government research grant money should be anonymous; the crux of the idea being that anonymity would encourage honesty in evaluation even when those reviewers might be competitors or might have vested interests. Thus the concept of anonymous peer review was birthed.

The idea of anonymous peer review was considered such a really useful idea that other federal granting agencies which were established later, such as the National Aeronautics and Space Administration (NASA) in 1958, wholly adopted the concept of anonymous peer review. Likewise, editors of scientific journals almost universally adopted anonymous peer review as a basis for making publication decisions. Anonymous peer review must have seemed an administrative stroke of genius, but its application would ultimately pervert and corrupt American science, bringing about the opposite of President Roosevelt’s and Vannevar Bush’s vision.

There is a major flaw in the blanket application of anonymity during peer review. If anonymity leads to greater truthfulness, then it could be put to great advantage in law courts. Courts have in fact utilized anonymity — in the infamous Spanish Inquisition and in virtually every totalitarian regime — and the results are always the same: People denounce others for a variety of reasons and corruption amongst the accusers becomes rampant. For decades, the use of anonymity within NSF, NASA, and elsewhere has been gradually corrupting American science. Unethical reviewers — secure, camouflaged, masked and hidden through anonymity — all too often make untrue and/or pejorative statements to eliminate their professional competitors. Nowadays, it is a pervasive, corrupt system that encourages and rewards the darkest elements of human nature.

If humans want to survive under adverse conditions, they adapt to their environment. And, survival in this current, cruel environment has led to a “consensus only” mentality. Scientists are quick to realize that

citing work that challenges the “consensus view” might result in their own reports not being published and their proposals for research funds garnering unfavorable reviews. Consequently, important scientific contradictions, if they can be published at all, rather than being discussed, debated, and subjected to experimental or theoretical testing, are selectively ignored. Suppressing and/or ignoring important scientific contradictions is like lying to the international scientific community and, in my opinion, is tantamount to perpetrating fraud on the American taxpayer.

For more than 50 years, American science has been infected by NSF’s instigated concept of anonymous peer review. The methodology of that institutionalization is fully described in an Office of Management and Budget report, dated December 15, 2004, entitled “Final Information Quality Bulletin for Peer Review”, which is downloadable from the following URLs:

http://www.whitehouse.gov/omb/inforeg/peer2004/peer_bulletin.pdf

http://UnderstandEarth.com/peer_bulletin.pdf

Eleven days after the report was made, I transmitted my critique of it to the White House where it made little impression. But the document does serve to detail the fallacies inherent in peer review in general and in the methodology of its application by the U. S. Government. It therefore seems appropriate to bring this brief communication to a close with that critique.

Recommendations for systemic changes in federal government peer review: a contrast to current procedures set forth in the office of management and budget

Final Information Quality Bulletin for Peer Review: December 15, 2004

J. Marvin Herndon, Ph.D.

Transdyne Corporation

December 26, 2004

Introduction

This Bulletin is a broadly based document pertaining to the federal government’s application of peer review in a wide variety of circumstances. The present document is specifically limited to peer review as applied to reviews of scientific grant/contract proposals, although there may be some carryover into other areas. As detailed in the Critique below, the procedures and methodology described in the Bulletin, together with omissions of substance, lead to a system of administration open for corruption. Recommendations are made for systemic changes to correct the present, seriously flawed system of administering peer review.

Critique of Bulletin

This Bulletin appears to have been crafted by individuals who either are extremely naïve of human nature or choose to ignore human nature (see Appendix). The Bulletin appears to be predicated upon the tacit assumption that in all instances peer reviewers will provide honest, truthful reviews. The tacit assumption that peer reviewers will always be truthful leads to a principal flaw of this Bulletin, namely, the failure to provide any instruction, direction, or requirement either to guard against fraudulent peer review or to prosecute those suspected of making untruthful reviews. The long-standing failure of the federal government to require and to aggressively enforce truthfulness in the making of peer reviews (1) encourages and rewards those who make deceitful peer reviews and (2) can be expected to have and to have had a deleterious impact on America's scientific capability, adversely affecting America's technology, and, concomitantly, weakening America's economic and military capability.

The Bulletin states, "... the names of each reviewer may be publicly disclosed or remain anonymous (e.g. to encourage candor)." The Bulletin approves the application of anonymity and even appears to promote some alleged virtue of its use, while being completely blind to its downside. If anonymity did in fact encourage candor and truthfulness, anonymity would be of great value in our legal system. There is in fact a historical record of the use of anonymity in courts: Anonymous testimony was used in the Spanish Inquisition and in nearly every totalitarian regime. In each instance, the results were the same: individuals denounce others, for a variety of reasons. Anonymity, instead of being a positive element, as implied by the Bulletin, is instead an extremely negative element, encouraging and rewarding the worst aspects of human nature and human behavior.

In the selection of participants in peer review, the Bulletin urges "due consideration of independence and conflict of interest" but, at the same time: (1) the Bulletin provides a highly restricted financial definition of conflict of interest, ignoring personal, professional, or scientific conflicts of interest, (2) the Bulletin falsely accords some individuals a conflict-of-interest-free-status, specifically, "...when a scientist is awarded a government research grant through an investigator-initiated, peer-reviewed competition, there generally should be no question as to that scientist's ability to offer independent scientific advice to the agency on other projects", (3) the Bulletin acknowledges that "the federal government recognize that under certain circumstances some conflict may be unavoidable..." and, (4) the Bulletin completely prevents the avoidance of conflicts of interest by approving the use of anonymous reviews, a practice which is wide-spread in federal government agencies that support scientific research.

The Bulletin fails to provide any instruction, direction, or requirement either to guard against fraudulent peer review or to prosecute those suspected of making untruthful reviews. At the same time, the Bulletin approves of the use of anonymous reviews that are free from accountability and from civil recourse. The Bulletin gives tacit approval to circumstances that allow conflicts of interest and prevents the avoidance of conflicts of interest. With that combination, the Bulletin encourages and gives free rein to any criminal or quasi-criminal element that seeks to attain unfair advantage through deceit and misrepresentation in the anonymity-protected peer review system.

The Bulletin states that “reviewers may be compensated for their work or they may donate their time as a contribution to science or public service”. All instances with which I am familiar, specifically, NASA and the National Science Foundation, do not pay reviewers for their time. Reviewers do reviews for a variety of reasons, such as to curry favor with agency officials or to exercise control over their competitors, but the bottom line is that time is money and agencies often get for their non-compensation a hastily done, superficial review.

The most serious short-coming of the Bulletin is its failure to recognize or to admit the debilitating consequences of the long-term application of the practices described above. The application of anonymity and freedom from accountability in the peer review system and the openness to conflicts of interest, gives unfair advantage to those who would unjustly berate a competitor’s formal request for research funding. The perception – real or imagined – that some individuals would do just that has had a chilling effect, forcing scientists to become defensive, adopting only the consensus-approved viewpoint and refraining from discussing anything that might be considered as a challenge to other’s work or to the funding agency’s programs. That is not science!

If a foreign power or a terrorist group had set out to slowly and imperceptibly undermine American science, I doubt that it could have devised a methodology for the purpose any more effective than the practices set forth in the Bulletin. Used for decades, these practices have diminished American science to the present point of approaching third-world status.

Recommendations for systemic changes in the administration of peer review

Like the Critique, the following recommendations are specifically limited to the administration of peer review as applied to reviews of scientific grant/contract proposals that are not subject to secrecy considerations related to national security and defense.

Generally, conflicts of interest, in the broadest definition, should never be permitted and should never be tolerated in peer review. Federal regulations permitting same, such as those under which NASA operates, should be changed.

Anonymity should never be used in peer reviews. Reviewers should sign and swear their reviews “under penalty of perjury” and should be held accountable for the truthfulness of their reviews.

Reviewers should always be compensated, and compensated well, for making reviews.

An independent “ombudsman agency” should be created to address conflicts and disagreements between the individual or organization submitting the research proposal and the agency to which the proposal was submitted. The “ombudsman agency” should be empowered to bring potentially unlawful activity to the attention of the Department of Justice for investigation and possible prosecution.

Recipients of federal research grants/contracts should incur the responsibility of making some pre-determined number of reviews under the conditions described above. Like defense attorneys selecting a jury, each grants/contract recipient should be accorded a number of “pre-emptive strikes” so as to be able to remove himself/herself from certain specific proposal reviews.

An individual or organization submitting a research proposal should be presented with an official list of names that the funding agency proposes to be peer reviewers. Like prosecuting attorneys selecting a jury, the individual or organization submitting the proposal should be accorded a number of “pre-emptive strikes” so as to be able to remove certain specific proposed reviewers.

Implementation of the above recommendations will begin to correct the long-standing debilitation of American science. The above recommendations are intended to correct the short-comings displayed in the Bulletin. Additional managerial improvements are possible, but are not specified in the present document.

Appendix

In 1623, Galileo, one of the greatest scientists of the millennium, precisely characterized human nature, especially the response to new ideas, in a letter written to Don Virginio Cesarini (translated by Stillman Drake): “I have never understood, Your Excellency, why it is that every one of the studies I have published in order to please or to serve other people has aroused in some men a certain perverse urge to detract, steal, or depreciate that modicum of merit which I thought I had earned, if not for my work, at least for its intention. In my *Starry Messenger* there were revealed many new and marvelous discoveries in the heavens that should have gratified all lovers of true science; yet scarcely had it been printed when men sprang up everywhere who envied the praises belonging to the discoveries there revealed. Some, merely to contradict what I had said, did not scruple to cast doubt upon things they had seen with their own eyes again and again....How many men attacked my *Letters on Sunspots*, and under what disguises! The material contained therein ought to have opened the mind's eye much room for admirable speculation; instead it met with scorn and derision. Many people disbelieved it or failed to appreciate it. Others, not wanting to agree with my ideas, advanced ridiculous and impossible opinions against me; and some, overwhelmed and convinced by my arguments, attempted to rob me of that glory which was mine, pretending not to have seen my writings and trying to represent themselves as the original discoverers of these impressive marvels....I have said nothing of certain unpublished private discussions, demonstrations, and propositions of mine which have been impugned or called worthless....Long experience has taught me this about the status of mankind with regard to matters requiring thought: the less people know and understand about them, the more positively they attempt to argue concerning them, while on the other hand to know and understand a multitude of things renders men cautious in passing judgment upon anything new.”

I have myself observed similar responses. Human nature does not change on a time-scale of a few hundred years. The peer review system cannot and should not ignore human nature.

**The state of the scientific research in Romania,
its causes and measures to be enforced to redress it
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Note: This text has been written at the end of 2004. Since then things have worsened. The main new element is an increase of up to 0.3% of the GDP which is spent on non-research costs in the name of research. This is associated with a tremendous increase in bureaucracy which cultivates fraud, corruption, degradation and destruction of scientific research in Romania.

Abstract: The scientific research in Romania is in a dramatic situation, following deeply wrong, anti-scientific and anti-social policies which are being pursued in this socio-professional domain.

The major cause of this predicament lies in the total inadequacy of Romanian politicians and research managers to understand the nature of scientific research and which is mainly due to a backward and retrograde mentality.

The state

The state of Romania's scientific research in the autumn of 2004 is disastrous. This predicament stems from deeply wrong, anti-scientific and anti-social policies that Romania has pursued and still pursues in this field of activity. In total disagreement with the policies of the developed countries of the world, Romania is forcing its scientific research into a methodical and systematic process of destruction.

The budget. In Romania, according to the budget law, the annual budget allotted for scientific research is 0.2% of its GDP, a totally insufficient amount in Romania where there are approximately 15-20,000 scientific researchers and a budgetary population of cca 2 million. An equitable distribution of funding must reflect this 1:100 ratio, which would amount to approximately 1% of the budget.

This deficit from 1% to 0.2% is not only a grievous social injustice but also an abuse with dire consequences for Romanian scientific research.

The illegal regime. Scientific research in Romania is carried out in an illegal regime. By the recently adopted law of scientific research, the scientific research would be entitled to a 0.8% budget, in approximate conformity with a fair allotment of the budget.

The above mentioned law is actually cancelled out by the budget law which provides for only 0.2%. Reciprocally, the budget law is itself cancelled out by the law of scientific research. These two contradictory laws, together with many others similar, throw the Romanian scientific research in an illegal regime and actually block up any kind of research activity.

One country and two systems. As far as scientific research is concerned, Romania is one country with two systems. According to the law, the system of national institutes of research does not have a budgetary funding. Also by law, these institutes are defined as economic agents and compelled to run marketable businesses. In these institutes the salarial income is paid irregularly, as it stems from the so-called research projects competitions and which are foreseen in the national plan for research and so-called core programs.

The institutes that are in serious budgetary difficulties are seen by the Romanian minister of finance as “black holes” of the economy, and as such, are being entitled by law to solely occasional subventions, like those provided by the so-called core programs. These latter programs provide a maximum of 50% of the last year’s turnover. If this amount drops in time, the government subvention will be phased out, in geometric regression, over few years. There is no precedent in scientific research, either in the world or in History, for salaries to be paid exclusively on a generalized project competition basis. In this respect, this policy of Romania's scientific research is an example of its professed aberration.

On the other hand, Romania is attempting to develop an artificial system of scientific research within various academy institutes and universities with research funds appropriated as supplementary incomes to the salary. These institutions have budgetary salaries and, in addition, they have an important income coming from the research budget. This double system regarding scientific research has deep, negative consequences: on the one hand, a false system in universities and academies get excessive subventions despite their poor quality of scientific research, and on the other hand, the national institutes face extinction, where, despite great difficulties, there is an outstanding and highly-efficient research that is being carried out.

Blockage of the laws. According to Romanian legislation, the national institutes of research in Romania are state-owned, but at the same time, the Romanian state is also the main beneficiary (or client) of the results originating from this research, and furthermore, the state is also the agent of those research activities carried out in these institutes.

As the owner is also the beneficiary and the agent, we are dealing with an impossible legal situation which totally blocks up the research activity. In a false and impossible legal and judicial situation, through its exquisite aberration, the contractual documents and norms are established by the state in its capacity of owner, accepted by the same state in its capacity of beneficiary and supported by the same state in its capacity of performer.

This is the accurate picture of a total managerial, judicial, administrative and legal blockage of the research activity in Romania, a picture that purveys the measure of the political vision and administrative capacity of the politicians and research managers in Romania.

Forgery. Romania has officially declared a number of 37,000 employees in scientific research, out of whom there are 23,000 scientific researchers; 8,000 “certificated” scientific researchers and 5,000 PhDs. It is totally unknown the difference between 37,000 and 23,000, and the difference between 23,000 and 8,000, and it is totally unknown the term “certificated” (is there “delegate” researchers?). With 23,000 scientific researchers, Romania is close to the European or US average of approximately 1 researcher per 1000 inhabitants. With 8000 “certificated” scientific researchers Romania is below the middle of this average showing a substantial shortcoming of scientific research. Other official figures, probably more realistic, give as approximate a number of 15,000 researchers or research employees, so the actual figure remains largely unknown. Besides these 37,000 research employees, Romania alleges to have about 20,000 researchers in the so-called departmental research, whose budget (another 0.2% of the GDP) is registered twice, firstly in the ministries budget and secondly in the science and technology budget.

Such contradictory and imprecise data reflect the whole ignorance that Romanian politicians and scientific research managers have in this field of activity, or their lack of transparency in a matter pertaining to the management of the public funds. In both cases the situation is not acceptable.

The destruction of the research capacity. Romania invests nothing in scientific research for a long time. The infrastructure of scientific research in Romania is notably deteriorated; the buildings, laboratories, utilities, lines of communication and access; specific equipment and technological facilities. The patrimony of the Romanian scientific research has decayed and has been largely plundered by the so-called privatization and “harnessed” by alleged research managers and/or by the socio-political power clans which they represent.

Scientific research in Romania is compelled by law to provide for the expenses of utilities and for maintaining the infrastructure out of the research funds.

Although the research equipment and apparatus have physically deteriorated long ago, the Romanian policy in science and technology does not approve for such structural spending.

In this gloomy landscape, here and there, a few laboratories have risen with small rooms, a corridor and a hall, all refurbished and air-conditioned with windowpanes, central heating, and equipped with up-to-date apparatus thanks to the generous gifts from Romania’s “generous” international partners.

Gifts that are allotted to “reliable” and “trustworthy cadres”. These shy “appearances” in the scientific landscape are real sanctuaries of modern scientific research in Romania; museums kept under key and entrusted (not accidentally) to some “researchers”. These centers are not used for scientific research activities but only for pretty pictures printed in political leaflets to be distributed at exhibitions, symposiums and “round tables” where the “successes” of the Romanian research are glorified, as for instance the “conro” products, meaning the products “conceived in Romania”. Under this political cynism, the suprastructure of scientific research in Romania, *i.e.* the scientific researchers, the research employees, have not the right to salaries, according to the law.

The onerous relation with European Union (EU). Romania contributes annually approximately 20% of its research budget to the EU communal scientific research, while the average contribution of the member states is only 5%. This proportion represents approx. \$15 million of the 80 million annual budget of the

scientific research in Romania. Only approx. 9% of these funds return to country. This contribution is set on the basis of 5% applied to 0.8% a budget, as declared by Romania and falsely accepted by EU. Indeed, 5% of 0.8% is approx. 20% of 0.2%. Thus, a policy of forgery in official documents practiced by both Romania and EU, prejudices the Romanian scientific research with approx. 1/5 of its budget.

Politicized international relations. Romania is wasting a substantial amount of its research funds by paying for the political contribution of a formal association with various international institutions and organizations of scientific research, as Dubna-Russia (some say approx. \$600,000 per year), CERN- Europe-Geneva (probably approx. \$3 million per year) etc. These expenditures are not justified by an actual participation of Romania in the scientific research performed in such institutions and organizations, but merely represent the simulated costs of public relations and other commitments of political nature.

Bureaucracy. The expenditures involved in the scientific research of Romania are increased by a factor of 3. This numerical factor—of social and political nature—derives mainly from the excessive bureaucracy and administration costs. For example, in compliance with such laws, the scientific research in Romania is defined as “service”, therefore, the costs are set by contractual estimates and paid off in a post-calculation system, after execution, which is a totally inadequate situation for the nature of this field of activity.

Corruption. Within the budgetary research system of Romania, specific to academies and universities, additional salary incomes are procured by law, like cumulative budgetary incomes (non-existent in any other state in Europe and in the USA), additional wage increments for PhDs, PhD supervision, loyalty, stress, etc., enormous discrepancies between the salaries of the university employees, while the national institutes of scientific research in Romania do not have salaries or such additional wage increments.

Furthermore, their right of supervising doctorate students has been cancelled by an abusive policy in favour of the universities. In Romania, the national institutes of research perform their activity on the basis of contracts, execution reports, competition, scientific results and scientific publications, while the budgetary system of Romanian research is remunerated with no contractual obligations. This double situation, established by the Romanian laws, generates abuses, falsehoods and corruption within the university system, academies and scientific research in Romania. Politicians in Romania are often professors, teaching at the same time in many universities; former cadres retaining a retrograde ideology who, by the help of Romanian political machine, have fabricated for themselves real university fiefdoms or so-called institutes of scientific research; political members of the Romanian Academy that have recently and abusively appropriated salaries based on the so-called merit. All of these are detrimental to functional, fair and honest social relations. Scientific research and university education in Romania is pervaded, suffocated and manipulated by fraudulent, abusive, and onerous political impostors among whom prevail the so-called managers who are the instruments of a persistent policy of destruction of this socio-professional field.

Waste, fraud and disorganization. By law, Romania is wasting its scientific research budget on the so-called scientific research programs with no real connection to scientific research and also of no intellectual, economical or social relevance. Thus, in the name of the scientific research in the national programs of research, Romania is financing alleged research in the fields like : “the inventory of gravestones in the Bellu orthodox cemetery”, “the menopause in rats”, “sculpture stone”, “history of painted funeral monuments”,

“matters of state aid”, “problems in the competitiveness of the Romanian economy”, “the issue of the virtual space of acquiring the European terminology” (CERES, Competition C4-2004), etc...

There are even more dramatic situations like in the programs of the so-called scientific research in Romania, like RELANSIN, MATNANTECH, VIASAN, etc.

The Self-promotion (based on the the so-called competition) of such aberrant research programs; the abusive lobbying and other onerous practices, is a serious threat to the Romanian scientific research and which are leading to its very own extinction. The so-called managers of Romanian research, namely directors, universitaries, heads, managers, coordinators, executives, monitors, reviewers of the research programme, jurists, accountants, etc. are incapable of managing the situation not only because this situation is virtually impossible to be managed but also because they are not professionals of scientific research. They are devoid of the professional competence required by any position of authority, responsibility and respectability. It is relevant in this respect the lack of an inventory of the scientific research in Romania, the lack of knowledge of the basic input data for analysing the system, like the research institutions, their mission, objectives, results, budget, financing, human resources, etc, indicators and prerequisites which are absolutely necessary for a possible attempt of managing this disastrous situation.

This deeply anti-scientific and anti-social policy is producing pseudo and anti-science, non, meta, para and quasi-science and false education, occult, mysticism, magic, aggression, mental disturbances, mental retardation, sub-culture, lack of civilization, a climate with a highly anti-social potential, illegality, wretchedness, primitiveness, social conflicts and dangers. Unfortunately, all these are done in the name of scientific research, thus putting the scientific research in Romania in a degrading and humiliating position such that it has become the main cause for young researchers to go abroad to developed countries and for depriving the research institutes of prominent researchers.

The overthrow of values. Except for Romania, all former communist states that underwent the socio-political changes at the end of the 9th decade of the last century, had a predominantly reformative part. Contrary to this general feature, the communist regime (so-called “pecerist”) in Romania crashed in 1989 under the weight of its own ineffectiveness and collapsed by the implosion of its own incapacity to carry on. While all other ex-communist states attempted a reformative follow-on approach, and a certain conscious control of these major changes, Romania “left itself adrift”, suffering a severe socio-political disruption. Also in this respect Romania is atypical, unique, unframable in any political-phrasable course, which proves once again the deep backwardness of the Romanian society. Consequently, in the situation of “carte blanche”, of “wholly and fully new”, ensuing the events in 1989, various political forces attempted to legitimize their authority in Romania by the sole natural means available in such situations, namely: instigation, terrorist diversion, blackmail, force - all these accounting for the tragic social events in the Romania of the ‘90s. The attempts of getting legitimate of the new power forces in Romania continue today. They will never cease until Romania will not realize and become conscious of her own social-political course, until it will not assume correctly her past and history in order to be able to identify her future options.

The socio-political powers, prominent to this day in Romanian society, are former structures of the political and secret surveillance's leadership of the society, broken up in small rival private companies which

are in competition on a free market and whose prey is the Romanian population. These small structures are almost patriarchal comprised of clans who have bonded together by their subversive past and which manage to appropriate themselves, illegally, of important fortunes. Gangs who fight a mob-like, fratricidal battle and whose main weapon is the mutual blackmail due to their own compromised past of cadres of the Communist Party (so-called “pecerist”), the Young Communist League (so-called “utecist”), the trade-union (so-called “sindicalist”), the state security (so-called “securist”).

The few independent individuals who managed to make fortunes in the Romanian society after 1989, presumably legal according to the new legislation but illegal according to the just laws, are subject to abusive attacks, in a mob-like pattern at the social level, by the breeding ground (“sereleuri”) of ex-cadres, “rats”, social surveillants, manipulators, diversionists, instigators of the past political regime and which add to the instability and social insecurity in Romania. This unceasing civil war existing in the Romanian society totally curtails any opportunity for Romania to jumpstart. Under these circumstances, Romania does not stand a chance of having either a responsible political class, or a strong middle-class, or a social functionality.

With upside down values, Romania is trying in vain to legitimize itself in her own eyes. This overthrow of values has substantially affected the Romanian scientific research after 1989.

The cadre policy of the communist regime in Romania implemented plenty anti-selection (or negative selection) in scientific research, in university education and academia such that “the upside down values” phenomena in these fields has grasped Romania and after 1989 has reached proportions beyond control.

Scientific research, academic institutes and university education were highly politicized in the communist Romania that was loaded with “on sight” political cadres and with hidden, “undercover” cadres exerting control and surveillance of people, in an attempt, possibly acceptable, to control the independent thinking of these intellectual environments (this falsifying of the scientific and professional values by the communist regime in Romania is unacceptable). After 1989 these cadres felt guilty and tried to fabricate a new, respectable identity to protect them from a possible “proletarian rage”.

Most likely such faulty “logic” remained only in their minds but the fear of these characters is an indication of the proportions (remained unknown to the general public) of the social, human damages and prejudices they inflicted on the general population during their subversive activity for a long time.

Their sole available weapon was mutual political blackmail and in the next ensuing months and years, following liberalization, the bribe.

The political blackmail and bribe which affected Romania right after 1989 occurred when a wide range of so-called academics, university professors, managers, etc., (many were absolute professional fakes) acceded to key positions of leadership of scientific research, university education and various academies, and who barricaded themselves inside these “armored institutional bunkers”. Once in power, such “paper” characters (not even “cardboard”) set the political machine in motion—fueled especially with illegal pecuniary means—towards the fabrication of many other similar cadres, alike false and detrimental, who were to strengthen the new structures as “reliable reinforcements”.

In the years that followed this “primordial” phenomenon, this “original myth”, there was a serious reaction to this anti-social activity (hardly now a “popular uprising” followed such provocative acts) and both the scientific research and university education in Romania were filled up with a bulk of “approximate” senior research scientists, “half-“, “one-fourth-“, “fractionary” professors. etc., who were moved up the ladder under the “popular pressure”. All former secretaries of the Romanian Communist Party (“pecerist”), all former members of the Young Communist League (“utecisti”) and trade-union (“sindicalisti”), all insignificant collaborationists capitalized on their status of loyal ex-“basic cadres”, on their contribution to the political masterpieces of the former regime and were promoted to high positions within the scientific research, academies and universities, even if they were devoid of the adequate professional capacity required to attain such positions.

Scientific research, academic institutes and universities in Romania are now witnessing and “enjoying” a heavy inflation of staff promoted on “socio-political criteria”, on syndicalistic, “socialist science” criteria, on the criteria of complicit underground consensus, of affiliation with committees, commissions and all sorts of other and other councils.

This generalized overthrow of values in the scientific research of Romania has disintegrated the fields of activity from the inside.

The “nestling” of these new “scientists, academics and university professors in their comfortable positions and functions resulted in the flight of young scientists abroad (the young run away from such despicable characters and from the disaster triggered by such “personalities”). It forced into “anonymity” the few prominent scientists who remained in the field and led to their gradual ostracism and banishing because the genuine scientific, university and academic activity is *per se* a constant “threat” to those impostors.

Such “delicate” situation runs the risks of being easily noticed and understood by young students doing research in universities, and for this reason, the key political point pronounced by the impostors in Romanian science are based on the slogans referring to the “young's mobility” and “international cooperation”.

In order to protect their illegal positions, these impostors who are clearly driving the young away from home, hypocritically complain at various “round tables”, and “under high auspices”, that Romania is “loosing its brains”.

Romania's brain drain is due to the fact that Romania has lost its “soul”, so the young flee precisely because of the wretchedness triggered by these characters around them, wretchedness that chokes to death the scientific research, the academic and university environment in Romania.

The trade in “scientific brains” and “intelligent organs”, or simply in “fresh fodder for scientific cannon”, that is practiced by these fake characters of so-called scientific research, in the so-called universities in Romania, this scientific trade which is complicit with the close “international cooperation” of their fake-alike counterparts “from abroad”, is a huge illegal business that destroys scientific research.

One of the most efficient ways these days of fabricating numerous scientific publications—masqueraded in journals of “international prestige” and “quoted on the ISI (impact science index of the Institute for Scientific Information) exchange”—is by trading in the young's work and manipulating the young' names in these publications.

As a result of this illicit business, the impostors of the Romanian science are manufacturing feverishly and frantically publishing trash which might account for their illegitimate positions. However, of special note is the fact that for an entrée at the “West” famous scientific centers, either as small-time researcher or as third- or fourth-rate researcher, one must have around them at least a “young man” (or a “young woman”) to be in harmony with the misconception of the “fresh blood” infusion.

The young in developed countries have seen better and faster the falseness of such positions, and are being ever more reluctant to enter this injurious and misleading game, so the only virgin territories remained to be exploited are the “intelligent Africa”, the “black territories”, as the sole fueling source for this political aberration and distress.

Teams of “experienced” recruiters prowl the university and scientific research territories of Romania ready at any time to throw the “lasso” around the native young and to enroll them in the “scientific research army”.

Impostors of the Romanian scientific research, academies and universities, both the higher-ups in leadership positions (thanks to their political cronies) and those in the “antique choir” (that is socially manufactured) keep feeling the incessant restlessness to legitimize their existence.

In order to fulfill this deep need, these false characters are continuously searching high and low “objective” criteria for promoting, legitimizing their positions, and finding automatic gizmos to grant them the right to such positions, and searching for magic mirrors where to reflect their image as “just” and legitimate.

The noisy and frantic gait in the pursuit of this chimera is probably the last activity performed by the Romanian scientific research personnel in the universities and academic centers in order to fulfill their turbulent existence (in a way heroic) in the Romanian territory.

The causes

The causes of this predicament in the Romanian scientific research reside in the retrograde and anti-social mentality of the Romanian politicians and managers, in their incapability of interpreting the status of this domain of activity in the context of the current epoch - as they are completely out of touch with the realities of this field of activity. In their incapacity for a positive, applied, empirical thinking (similar to the scientific method), in their lack of intellectual exercise and, finally, in their complete incompetence and ignorance. All these regrettable peculiarities are effective in the so-called Romanian policy in scientific research, which leads consequently to the destruction of the field.

Research-production confusion

Romanian politicians and their managers are seriously mistaken about scientific research, not only for industrial, manufacturing, agricultural, etc. production, but also for trade activities and services of all kind. Scientific research is that specific, a distinct and well-defined activity by which science is produced at social

level. Science is the total sum of positive knowledge, namely from those that may be considered more likely as being certain, reliable. A huge amount of such knowledge pervaded the stream of history long time ago and keeps entering the general social use. Such knowledge became and still become popular especially due to the activity of engineers, technicians, practitioners, of popularizers and coarseneres of sciences.

The remainder, embedded in the traditional body of sciences, as well as the new ones provided by recent scientific research, are generically used as scientific knowledge.

This scientific knowledge is fundamental or basic insofar as it refers to scientific laws and principles, is applicative or applied when it aims at particular scientific situations and technical or technological insofar as it is mature enough to present a potential of technological transfer to the production. Accordingly, the scientific research is also fundamental (or basic or advanced), applicative or applied and technical (technological, or aiming at technological development). Scientific research is a tremendous resource of knowledge - which proved and still proves its social usefulness, unlike production, commerce, services - which are the practical result of physical goods and products of social usefulness. These two fundamental activities, science and production, exist in the socio-economic life of any human collectivity and they are by and large poles apart.

Given this situation of “mistaken identity” existing in Romania in this respect, it is absolutely necessary to fully understand the fact that scientific research equals scientific knowledge, unlike production which means physical product in a broad sense (namely industrial, manufacturing, agricultural, commercial product, service etc.).

Scientific knowledge is produced by scientific research, by scientific experiments performed in laboratories, by imagining and testing scientific theories and is freely provided to the general public—by scientific publications in the case of public scientific research, or to the employer—in the case of private scientific research or with limited dissemination. Production, broadly speaking, is based on execution projects, business road-map and is put on the market by economic agents. Mistaking one for another leads to a blockage in both activities. Inserting economic terms in scientific research—widely practiced by Romanian policy—such as: turnover, good, capital, price, supply and demand, business road-map, managerial plan, profit, economic impact, social impact, estimates, invoices, etc. it is not only improper—as such terms by their nature are not applicable to research—but is also deeply detrimental as it blocks both the research—unable to perform its specific activities in such terms, and the production—which facing such aberrant policy may await erroneously for the research to accomplish its mission.

Technological transfer

Technological transfer is the activity by which the resources provided by the scientific research are used as main ingredients in producing material goods of social utility.

Technological transfer is made by economic agents who harness the resources of scientific and technical knowledge from the scientific research and develop them by “casting” into physical, commercial, end-, marketable products.

Technological transfer is an activity specific to economic agents, of interest especially in advanced technologies, and it involves investments, market analyses, increasing of cost efficiency; all these activities are of economic nature and by no means of scientific one. The technological transfer is made by economic agents and not by the scientific research. This fact, fundamental in the socio-economic development of a country, is not understood, accepted or promoted in Romania.

The technological transfer is made, mainly, by privatizing scientific research, by economic agents developing their own scientific research or by transacting the research patents.

Insofar as Romania sincerely wishes to commit itself to the route of socio-economic development then it must understand that it is necessary to cultivate, promote and develop scientific research, by assuring the required expenditures for infrastructure, equipments (structure) and salary funds (overstructure) from the budget and to uphold and promote the economic agents for them to take over the resources of techno-scientific knowledge supplied by the scientific research. The Romanian laws must be positive for the economic agents in Romania and not negatively directed against the scientific research in Romania. A policy that destroys the scientific research, as the one practiced by Romania, implicitly destroys the technological transfer, and also the technological production and development by eliminating the resources of scientific knowledge which feed off these activities.

Romanian politicians frequently abuse the slogan “the jump start of the Romanian economy”. “Romanian economy” has ceased to exist for a long time, the state economic sector is ever more diminished in Romania, the natural tendency is towards emphasizing the private economic sector. Romania is a country open to the world economy with a free market and the economic businesses undergoing on the Romanian territory insofar as they need to incorporate the scientific research in Romania, they do it by themselves and do not ask the indigeneous scientific research to self-incorporate in them.

By the “jump start of the Romanian economy” Romanian politicians do not address, unfortunately, the scientific research nor economic agents. By this empty formula they are speaking in vain. This slogan is void of any substance as it can not be applied to something, it has no applicability. It is a mere theoretical, ideological and demagogical formula. In this context, all that Romanian policy could sensibly do in scientific research, is to cultivate it as to make it able, besides its own development, to generate as many firms - state owned or private, businesses firms of advanced technology - as possible, and to foster the rise and development of such firms so that they become lucrative.

Regrettably, by its policy Romania has decided to take the opposite route, by destroying its own scientific research and at the same time, by blocking the emergence of businesses of advanced technology.

Motivation

The funding of the scientific research must come from the budget, mainly from governmental institutions, since this research provides scientific knowledge which is a value of general social usefulness, the resources from which advanced technologies and technological economy are fed off and also the core of education, training and scientific education—by which social stability and superiority, specific techniques,

methods and procedures of scientific nature (scientific method) are acquired, it also provide the capacity of managing the social, economic, financial and military crisis and finally, of relevance in the current epoch, it forms the basis of military supremacy and social protection in the fight against terrorism. It must be overemphasized that terrorist fighting methods are actually based on the product—out of control—of the scientific research, including both scientific and technical results and also highly-qualified scientific and technical personnel; consequently, the sole possibility of countering the terrorist attacks, in this epoch, lies in resorting to appropriate technical and scientific knowledge and to find competent human resources in the scientific research.

Of special note in this context is the fact that the great powers of the world, like the US, explicitly motivate the funding of scientific research based on intellectual and military supremacy.

Much too often, sincerely or not, but definitely erroneously, Romanian politicians raise this wide scientific and technical gap existing between Romania and these powers, to justify the abandonment of the scientific research. This political stance is fully detrimental if Romania wishes to continue its existence and survival in a world dominated by great industrial, financial and military powers. Hence, it is imperiously necessary for Romania to profess the same technical and scientific methods as those on which these powers are based on, in order to have at least a common language and orientation, an adequate, positive and constructive position in this world's political context. Giving up science and technology would mean for Romania, giving up its existence on this planet. Having a scientific research conducted adequately, properly and in accordance with its nature, is vital for its existence.

It must be also underlined that the great powers direct their scientific research not only into military supremacy but also into “intellectual supremacy” which provides good opportunities even for small states, including Romania, on condition that they pursue an adequate policy in the scientific research. It is noteworthy for Romanian politicians, that negotiations are ruled by intellectual supremacy and not by military force.

On the other hand, Romanian politicians must understand that NATO partnership is viable and advantageous both for Romania and NATO, if Romania enters into the arena not only with a geo-strategic position and political loyalty but also with intellectual, scientific and technical values.

By the same token, joining the European Union enhances its value if Romania presents a superior intellectual, scientific and technical potential as a pledge for, and from which may derive, a rational administration, a management of the economic process, a social control and stability—essential European values. The financing of the scientific research does not rest on the shoulders of the economic agents in any of the developed states of the world, as these economic agents are not—and cannot be—interested in acquiring scientific knowledge, for such knowledge is not—and cannot be—marketable by itself. In developed states of the world, the funding of scientific research is practically, entirely provided by the state, taking into account its superior development, so that it may become attractive for such economic agents as well, which then, might become interested in buying and jointly capitalizing the results of such research.

Sadly, Romanian policy on this matter is antagonistic to these current, rational and adequate practices whose effectiveness is fully confirmed.

Empty formulas. Social command. Regarding the scientific research, Romanian politicians talk and act improperly, inadequately, contradictory, insincerely, demagogically and illiterately.

If some people often grieve the techno-scientific gap between Romania and the developed states of the world- as a pretext for abandoning scientific research in Romania- many others, or even the same, often alike, formulate aberrant demands to the scientific research, as its involvement in economy, society, education, art, sports, culture, policy, in oriented, competitive, pre-competitive, industrial research, innovatics, inventics, etc. All these are empty formulas or at least commonplaces, trivia whose purpose is to hide the incompetence, the lack of sincerity and to increase the false socio-political costs of the bureaucracy and inept administration. The scientific research is naturally “involved” in all these, it has a tremendous social, economic, intellectual, cultural and civilizing relevance, therefore this “involvement” cannot be an issue on the political agenda unless one wishes by all means to exercise dishonesty and futility, a superfluous gymnastic practiced by Romanian politicians and research managers.

Romanian politicians must understand that they are destroying the scientific research in Romania due to a deeply wrong policy, which must be immediately stopped, that they must give up and they must foster, promote and develop scientific research in Romania according to the nature of this field of activity and not to trash it.

They must understand and accept that, once things are improved, assuming they will ever be, we cannot ask the scientific research to go beyond its level and real possibilities, that, in this respect, things must be considered according to their actual state, that all we can do is an attempt to re-normalize the route of the scientific research and then wait for it to become fruitful, as it was in the past under favourable conditions and as it is in other parts of the world where such conditions are already a reality.

Such policy, relatively “economic” at a glance, which does not consist of verbal or administrative frenzy, ideological rhetoric, bureaucratic initiatives or verbosity, would already be an enormous contribution to the adequate status of the scientific research, whose principle of evolution—validated by historical development—is property, adequacy of the political context and full freedom of action.

This principle has already been assumed by developed states since 1945, and was distinctly expressed by Vannevar Bush: *“scientific progress on a broad front results from the free play of free intellects, working on subjects of their own choice, in the manner dictated by their curiosity for exploration of the unknown”* (“Science: the endless frontier”, 1945, report to the president of the USA).

Putting this principle into practice has led these states to their current situation of superiority, therefore it sets a cardinal example to be followed by the Romanian policy as well. It must be highlighted that this principle does not foresee plans, programmes, contracts, execution reports, turnover, etc., for scientific research, on the contrary it stresses the importance of the free intellectual movement “for the exploration of the unknown”.

As opposed to these positive, constructive and effective orientations, Romanian politicians cynically and incessantly rest the blame on the scientific research, in an aggressive manner, specific to incompetence, abuse, fraud and imposture.

The dictatorship of the “democracy” and the “democratic” terror practiced by this Romanian policy of scientific research, the complete politicization of this domain was and is still “fruitful: “the fruit” being the destruction of the scientific research.

The aberrant policy of scientific research conducted by Romania in the past, is being continued to these days in monstrous forms by false ideologists, by compromised characters (“peceriste”, “uteciste”, “securiste”) who do not accept that in their capacity as politicians and scientific research managers, they must be “public servants” and put themselves at the service of functional, democratic ideals of a modern society according to the legitimate, social aspirations in the current epoch and they should not be frustrated and nostalgic dictators. Unfortunately, it seems, however, that in Romania “communism definitively conquered in towns and villages”, the “dictatorship of the proletariat” is now the “dictatorship of the democracy” which accounts for the current predicament of the country, and together with it, that of the scientific research and the science education.

The diversion of projects and competition. Romanian politicians and research managers, often assert erroneously, that scientific research in developed states of the world is performed on the basis of projects and competition. This statement is false. Scientific research in these states is carried out mainly in governmental and state institutions (national laboratories, institutes, foundations, councils, etc.) based on a budgetary funding covering the necessary costs for infrastructure, research equipments, manual labour (meaning salaries) and other additional expenses. This research—public or governmental—is finalized with scientific results which often (but not always and not necessary) are disseminated in freely available scientific publication, and is reviewed on a regular basis by scientific experts who make recommendations concerning its future course. This research is not conducted on a project basis, nor does it involve any competition except for the natural, professional one for achieving a scientific, important, valuable result. The top product of this research is the scientific result and not the scientific publication, albeit the results are always displayed, presented, published, etc., and the appraisal of this research does not concern its re-dimensioning but a possible re-orientation, re-placing and mostly an improvement of its management. In these developed states the audits are never conducted on the research itself but on its management, unlike Romania, which always aims, in such situations, at the scientific research itself and never at its managers and politicians. The chief priority of all people involved in such a research is improving and providing a high-quality human resources, an efficient spending of the research funds, all with a view to achieving superior scientific results, namely new and correct results in the scientific domain.

Within this main research in developed countries of the planet, it does not exist—and has never existed—the concept of competition-based remuneration, as is destructively practiced by Romania in its national institutes of scientific research.

Developed countries of the world also conduct (in universities) a research smaller in scope but qualitatively significant. In these places of the world, the cardinal mission of the university staff resides in providing an education, training and scientific education of a high standard, and additionally, carrying out research activities.

Since the funding of these research activities are not always and everywhere foreseen, their funding is often obtained by projects and even competition.

The funds deriving from these projects and competitions cover the equipment, mobility expenses or the employment of temporary staff—usually doctoral or post-doctoral students. The salaries of the university staff involved in research activities are paid from the budget—for their teaching activities—and not by research programs. Major projects are allotted, often by competition, to state or governmental institutions—in which the main research is carried out—but these projects never contribute to the salaries of permanent staff which are remunerated from distinct budgetary funds.

In this situation, the funds resulting from projects and competitions cover mainly, the equipment, infrastructure expenses. In all cases, the projects and project-based competitions in scientific research are intended—in developed states—for obtaining funds to cover additional expenses and by no means for salaries.

Romanian politicians and research managers, although travel a lot in developed states of the world, are not able, unfortunately, to notice such realities, furthermore, they return with blatant aberrations on such things, that have no connection whatsoever with the reality, and they refuse to hear the accurate accounts of the genuine researchers who have actually worked within the research systems of the developed states and who speak, accordingly, from their own experience.

The university and academic diversion. Developed states of the world cultivate an active university research with long historical traditions. Like the other ex-communist countries, Romania, does not have a tradition in university research, due to historical reasons, but has a considerable one in advanced scientific research, with noteworthy results—that are known all over the world for a long time, in scientific research circles in national institutes.

Scientific research is structured according to its various disciplines and fields of activity, with physics at the top of list, since this science is not only highly mathematized (thus guaranteeing positive knowledge on natural phenomena) but also one of greatest social impact, like nuclear energy, electronics, laser, advanced materials, spectroscopic techniques, communications, electronic computation, etc.

Romania is world renowned in scientific research, particularly in electronic computers, lasers, in notable contributions to the technologies of command and control of the nuclear power plant in Cernavoda, in important contributions to the fundamental, theoretical and applicative knowledge in atomic, nuclear, solid state physics, in connected fields (chemistry, mathematics, engineering) all developed at Magurele-Bucharest.

At the same time, during the years, Romania developed an important scientific research in chemistry with remarkable results in plastics, polymers, in the field of engineering and technical sciences, electronics, mathematics, biology, etc., both in Bucharest and in other places of the country, in prestigious scientific and technical research centres as Cluj, Timisoara, Iasi, etc. All this research have been performed in Romania's national institutes of scientific research, which are, at the present, subject to a continuous and persistent process of destruction due to the disastrous policy run by Romania in this field of activity.

Due to an antagonistic policy, triggering and fueling social conflicts, Romania attempts with little success to oppose (in an abusive, provocative, diversionist and anti-social manner) university research—hastily, inadequately and unsuccessfully improvised—against scientific research at the national institutes, with the idea to build up an approximate and acceptable image in the world. By a frantic activity of confectioning a fake and inconsistent image, Romania managed to have, these days, more universities than university professors, and more centres of the so-called research than university researchers.

This false and injurious activity is nothing but an attempt to build up a capital of image which may mislead for the moment but not in fact and on a long term. However, in Romania, political cunning, improvisation and lie are, sadly, much too frequent and also much too assumed and professed. The university research is an important field of activity, which must be giving all due consideration, which must be cultivated, developed and promoted by providing competent human resources in the scientific research, by investing in research equipments and university laboratories, by relieving the university professors of excessive teaching activities. Under no circumstances, the huge disparities in salaries among university staff, recently adopted by law in Romania, nor the destruction of the national institutes, will lead to a genuine university research.

Furthermore, Romania finds itself in another peculiar situation. The former Soviet Union attached great importance to academic research. Such a research was not existent in Romania, due to historical reasons, but existed to various extents in the other countries of the former socialist camp. Such academic research was virtually unknown in the developed countries.

After the events in 1990, Romania has decided, under interested political and pecuniary influences, to develop a so-called academic research—mainly in the Romanian Academy, strictly pursuing the Soviet Union's paradigm—which has set up approximately 60 institutes with about 4000 employees overnight. Despite the fact that this Academy is an “autonomous and independent” institution, its funding is approximately 25% of the national science and technology budget. Such an artificial, false situation is highly detrimental, as it promotes the image of a cardboard scientific research, confectioned by the political machine, growing up by spontaneous generation—like the mushrooms—which entirely falsifies the image of the scientific research. Many of the so-called institutes of this academy keep themselves busy with trivial, politically commissioned and controlled matters, such as: “the quality of life”, “revolution theory”, “politology”, “intelligence”, “totalitarianism”, many of them parallel, and conducted directly by politicians or right under their influence. This research does not yield any noteworthy result (nor could it do so, being outdated, improvised and with no connection to the current scientific issues) but in turn, feeds off and cultivates an ever-pervading corruption, abuses and frauds. Most of the members of this so-called academy have a membership allowance, a merit-based salary, a salary as a director or budgetary researcher in the institutes of the academy, a teaching load (or many) as professor and additionally the retirement pension, since many of them are retired. All these are budgetary incomes, which is a waste and an illicit situation of artificial social discrepancy. There is no similar example in the world of a member of an independent organization being paid from public funds only for its membership status. The Romanian Academy is unique in this respect. And sadly, is declaring itself “everlasting”.

EU diversion and the international cooperations. The European Union's budget for science and technology is only 5% of the science and technology research budget of the member states. Accordingly, this communal research is minor and mediocre, a laymen-like, therefore the demand formulated to the Romanian scientific research of "joining" it is purely political, with no applicability. Romanian scientific research has for a long time been integrated into the global and international scientific research by the results achieved over time, by developed cooperations, by scientific publications; like European research, it is not integrated either into the communal one (nor could it be). It is impossible that one demands an important, major, substantial budget-based scientific research, to align with a minor one, as the communal research is, and to be encompassed in it, so much the more as this research is highly politicized and ruled by bureaucracy, empty verbosity, lobby and lottery. The leaders of the communal research are not who's who of the world scientific community, many of them do not even have a scientific professional background, the research themes of the so-called communal research do not derive from the science body but, unfortunately, from a low-political vision, improper to science.

The communal research is a falsehood conducted in the name of science. In 2004, there were returned to Romania \$5 million of its \$15 million annual contribution to the EU research, hence, it is obvious that this contribution was set irrespective of a sound analysis of the preliminary research plans.

Another political slogan of Romanian politicians and managers is "international cooperation". Scientific research naturally seeks international cooperation which must be cultivated, fostered, promoted, developed, they cannot be forced upon, for in research not everybody can cooperate with anybody, anyhow or anytime. Scientific research must be developed to naturally produce international cooperations, as Romanian scientific research produced over the years; a high-quality research will never be performed by simply imposing national or international cooperations.

Researchers compare notes on a regular basis and this exchange of ideas, standpoints, opinions result in scientific cooperations. Scientific research is not a routine work, a co-op with quantifiable procedures that could be multiplied by cooperation or advanced by increasing the number of "workers" (if one worker dig a ditch of one cubic meter in an hour, sixty workers will not dig that ditch in one minute").

Pushing the politicking into research cooperation and even conditioning the research projects by the number and scope of the international cooperations—common practice in Romania—are actually intended to hide scientific tourism—a classical example are the so-called plural international PhDs, masters, etc. supervision.

Romanian politicians must accept that scientific research is a distinct, well-defined socio-professional domain, with its own peculiarities, norms, laws, dynamic and logic which cannot be shaped by improper policies. Hence, the fiasco of Romanian politicians and research managers as against to Romanian scientific research which continues to exist, albeit with enormous difficulties.

Scientometric diversion. Romanian politicians and research managers erroneously believe that the scientific value of the research resides in the quantity of scientific publications and number of quotations. These two grades, incidental to the scientific research, are quantified by a factor called "of impact" pushed forward by a trade company from Philadelphia-USA, namely ISI Thomson, which allegedly based its

theories on a hypothetical science named scientometrics. In fact, ISI Thomson makes publicity commissioned by publishing houses and interested institutes of scientific research and taking advantage on this occasion, also an unrequested publicity for many researchers, research institutes, scientific journals, by publishing the list of publications—which is an illegal activity.

By this commercial activity ISI Thomson falsifies the relevance of scientific researches, confectionates, manipulates and distorts the public image of the researchers and of the scientific results and practices an unacceptable aggression upon the scientific research. In developed states, ISI Thomson, the impact factor and scientometrics are actually scarcely employed, and solely as a benchmark—and a questionable one—for effectiveness and costs in scientific research. As in many other dire errors professed by Romanian politicians and research managers, in this case also, they uphold as absolute an impropriety, assuming once again, an unique position in the world. Fact is, that in Romania, the “ISI conception” remains exclusively declarative with no significant practical application. The same holds true for EU communal research, where it is not implemented, although the political commissioners of UE communal research trumpet it with much ideological and propagandistic vigour, and also with much militancy. These commissioners have themselves no kind of “ISI”.

An excessive number of scientific publications reflects an incorrect approach to the subject, wrong or incomplete results, anyhow, a rough research, actually failed. The fortunate cases are abundantly parroted, incessantly compiled, with no touch of originality, with no note of new and correct in science.

An excessive number of quotations denotes, at its turn, that the quotations are improper, with no connection to the subject, or, if proper, it shows that the quoted result is, in fact, imperfect, erroneous. In all these situations, scientometrics and the impact factor mirror but the scientific sub-values and actually legitimate the impostors in scientific research.

“Violin is not the musical instrument on which one beats the drum on the back”, but has a whole different logic. This impropriety is the final say of scientometrics on scientific research and its results. Unfortunately, upstanding professionals in Romanian scientific research, respectable but naïve, make often the scientometric confusion, due to a desperate and legitimate attempt to stop the ascent and invasion of the impostors in research but without noticing that the impostors’ blockage is a political matter and not a professional one. (Professionally, the impostors are blocked by themselves).

The illegitimate status

The impostors illicitly reached high professional, managerial and political positions in scientific research. Academia and universities are “eaten”, “dried” by their status of illegitimacy, false position, by their frustrations of non-, anti- or pseudo- science. The public at large can easily ascertain that by simply watching TV, the uncertainty of their speeches (in the rare and unhappy occasions when they cannot bypass the public). In their precarious capacity of a logical communication, in the affects of their verbal communication system.

To cure their “disease” they are frantically searching for definitive, out-and-out lists of objective criteria, to justify their false scientific positions and to pledge for further promotions to higher “chairs”. Naturally they are in pursuit of a chimera. They have heard, sniffed something but they do not know, understand, remember what they have been told, show, offer and what they have refused. Genuine professionals in scientific research, university professors of authentic vocation are not concerned with such counterfeit professional promotion, their criteria are themselves and their activity. The scientific, professional, managerial and political values in scientific research, in universities and academia are exactly themselves and their professional results. Generally, the inventory of professional activities in these domains takes account of the scientific results, scientific publications, scientific papers, public presentation of these results, curriculum vitae, and as it does concern the professors, according to the quality of their “product”: the student, the graduate.

The attempt of leading perfection to impossible limits in research and education, of altering the nature of such activities according to the metaphysical “concepts” of one or another remains elusive and the tailoring of the scientific research and the teaching act by the improper position—pattern held by these science impostors, remains hilarious.

So, either the scientists are genuine, and in this case, they know it is impossible to formulate such steady, absolute and universal criteria, or they are impostors and then, whatever criterion they formulate it is vitiated and vicious.

Of course, the thirstiest for justice are the guilty ones. They would like to overthrow the current false pyramid of professional positions in Romanian scientific research, universities and academies and to put it back with them on top of it or at least, in a little higher position than they are now. “Who is judging who?” is the battle cry in this pathetic war within scientific research, universities and academies in Romania. “To be rejected but based on an objective judgment, and if admitted, be sure this judgment was objective”, this is the present-day formula in Romania on this matter. Unfortunately, this is not the route forward. The solution is pure and simple political. The utmost one can sensibly do in this direction and that would be of a huge benefit, is to rule out the improper measures in scientific research, university education and academia and to set up an adequate policy in this areas of activity. The immediate consequence would be a self-expelling of the impostors and their placing—accepted by the majority and acceptable as it is right, convincingly “right”—on natural, professional positions.

The impostors in Romanian science feed themselves, suck their sap, their substance, fuel themselves from the misery and the disaster triggered by a totally insufficient budget allotted to scientific research, from alleged laws and legal norms- contradictory, ineffective, fallacious, outrageous by their ineptitude, from administrative and financial-accounting norms—incomprehensible due to their abundantly clear lack of grammar, from wide-spread corruption, abuses and fraud practiced in this field by the so-called state authorities in Romania, from confusions, idle formulas and managerial slogans in the competition and project-based system in scientific research, from the law-protected absence of salaries, from the management, administering, evaluation, monitoring, carrying out the so-called national program of scientific research where trade in influence, illicit incomes and falsifying of the scientific research are flourishing. All

these issues are purely political and only policy can banish them. When leaving the impostors in Romanian scientific research high and dry without the “object of work” and all these illicit sources of income, they will lose “interest” for science, profession, research and research management and fall off like dry woods. By the same token, the weighty higher-ups in universities are “feeding off” exclusively from illicit incomes extorted from students. The determined measures for eradicating this plague are also of political nature. When they will no longer take teaching bribe, university kickbacks and academic graft, such characters will suddenly find the professorate as not interesting, the teaching activity tiring and they will apotheotically fade away, leaving behind them a black trail.

Unfortunately, Romania has a theorizing people. Instead of working, Romanian people count the works and theorize on work. Instead of running businesses, the Romanians theorize on the most profitable businesses. Instead of managing, the Romanians teach how to do the best management and the best policy. Frustrated by a history that deprived them of conquests, war preys or socio-political edifices, the Romanians offer themselves a compensation for their dissatisfaction by playing nine men’s morris and laying down on paper strategies. With our “sportsmen” and “young Romanian scientists” who sweep the board, obviously “abroad”. With our “bravehearts” and all the others—so many—who were well-nigh to win a Nobel prize but end up being “eaten alive by the foreign competition”. Being actually defeated, the Romanians find an outlet in dreaming. Romania is a “dream country” meaning it has an oniric people. The Romanians suffer from the competition, the best one, the categorizing, the hierarchy, the polls and gambling diseases. Romania has a prize-winning people, obsessed with success. This sporting vision of a people on the Grant Bridge (a famous bridge in Bucharest) moves inherently into the scientific research. (After all, Paris was also conquered by “the French”). Scientometrics and the impact factor, quench the thirst for absolute, paradox and “primus inter pares” of the frustrated Romanian pseudo-scientists and give the Romanian scientist a “make it big” and “didn’t live for nothing” impression. All alone in their sports games portfolio, the intelligence coefficient remains still alien to Romanians and Romanian scientists, which seems perfectly understandable. Romania has a meta-existential people living in a virtual world. The Romanians are not actually living, but live under the impression they do that.

There is no Romania but solely the idea of Romania. And this fully inoperative mentality is amplified and exponentially increased by Romanian politicians and managers.

Measures

When facing the dramatic situation of the scientific research in Romania, following a deeply wrong, anti-scientific, anti-social policy pursued deliberately, methodically and consistently by Romanian politicians and research managers, a set of **measures and procedures** has to be enforced. Such measures must not necessarily and programmatically be aimed at redressing the scientific research in Romania, as such redressing, if and when it occurs, will pertain to the inner logic of this research. On the other hand, not only in Romania but also in other parts of the world, the views and opinions concerning this redressing are far too different and sundry to successfully make up a consistent vision. In particular, the general public, the society,

researchers and research politicians of developed states of the world, are often dissatisfied with their own research and to put forward a set of measures does not always seem to be fruitful and meet the expectations. The best and efficient “policy” of scientific research seems to be to allow active and competent researchers to have the opportunities, under a favorable political and managerial climate, to carry on their scientific research. Unfortunately, such a “policy” is quite hazy on its practical determinations so that, one must accept it solely as a general desideratum. The main target of some practical and operative measures in organizing and managing scientific research in Romania must be: a radical and definitive abandonment of the disastrous policy pursued in this domain, an honest and determined attempt to change the retrograde mentalities triggering this inept political conduct.

When facing the grievous policy of Romania's scientific research one single measure must be enforced: “Stop the disaster!”. Naively, the best research policy in Romania would be to forbid politicians and research managers to ever lay their hands on scientific research, to ever make decisions, to enforce laws, directives, norms, rules and orders in research.

Left at the mercy of a budget, even totally insufficient as it is at present, but freed from the aggression and destructive action of managers and politicians, the scientific research in Romania would be more successful compared to the destruction it is nowadays subjected to.

Given this sad state of affairs in Romania, a list of appropriate measures in the field of scientific research shouldn't include “what must be done” but rather “what mustn't”.

Nothing of what is now undertaken, regarding the organization and management of Romanian scientific research, should be undertaken but everything should be forbidden.

With respect to the existing situation, at present, the most adequate policy in the scientific research in Romania is an anti-policy, the one and only anti-policy able to assure a certain survival. The measures that must be enforced while facing the disaster of Romanian scientific research are “negative” measures.

The suspension of current legislation. Romania must immediately suspend the current legislation in research, in particular the so-called national plan of research, the geometrical- regressive “core”, the provisions of the financial-accounting law, it must suspend the status of the economic agents ascribed by law to the national institutes of research, and to stop the payment of political contributions to the EU and other international organizations.

Emergency normative regime. Simultaneously, Romania must provide in an emergency and transitional regime a basic budgetary financing to fully cover up the expenditures in scientific research.

Reorganization. At the same time, Romania should proceed immediately to have an accurate, complete and transparent inventory of all its research institutions including institutes, centres, laboratories, companies, collectives, teams, academies, etc., even of individual researcher; to have a precise knowledge of its research capacities, human and material resources, their condition and also of the experience, capabilities and functionality vistas. This inventory should be based on an in-depth reorganization that must aim firstly at re-harnessing the potential existing in Romanian scientific research, framing the research—as highly as possible, into the genuine course of scientific research and placing it—as soon as possible, on the route of international research and functional directions-oriented research. All these steps must be taken in

consonance with the possible natural state. Obviously, such an organizational analysis cannot be undertaken but only by professionals of scientific research—as this activity assumes scientific method. It must be immediately summoned and gathered in a national commission with the purpose of collecting all the proposals of the organizational and administrative measures and to implement them.

Fundamental reference points. The key conclusions of such a restructuring and reorganization must result in a 0.8% budget for science and technology in Romania, ruling out not only the double system in scientific research, corruption, abuses, fraud, bureaucracy and impostors in scientific research but also the politicians and politically confectioned, the non-scientific research managers, while implementing a clear-cut legislation adequate for the nature of this field of activity, clearing up Romania's international relations in scientific research and taking immediate steps to re-define science education in Romania.

All these measures are perfectly possible because there are in Romania (still are!) outstanding professionals in scientific research who can formulate and implement them. It must be stressed that such an action is strictly political and its success rest on the success of the Romanian policy in reaching these prominent researchers and motivating them to assume this mission.

Scientific and Political Elites in Western Democracies¹

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Abstract: Examples are examined where science conduct falls far short of the ideal. Similar failings in political processes are considered. The question is asked whether there are common roots to these failures and if so how they can be corrected.

Evolution of an elite into an oligarchy

In the 1940's the largest telescope of its time, the 200-inch at Palomar, was conceived and built. Since Rockefeller and Carnegie were rival capitalists the Rockefeller Foundation could only give the money to California Institute of Technology rather than the Carnegie Institution of Washington where the world's leading astronomers were. Cal Tech, however had no Astronomy Department so an agreement was signed between the two Institutions that they would jointly operate the Observatory. The noted Carnegie astronomers such as Hubble, Baade, R. Minkowski then initially used most of the telescope time. Younger staff members were gradually included.

Quasars were discovered in 1963 and astronomers rushed to observe them because they assumed their high redshifts meant they were at great distances and that the nature of the universe would thereby be revealed. The Cal Tech radio astronomer who isolated the positions of the first quasars asked for telescope time to observe their spectra and obtain their redshifts. He was told only certain of the faculty could observe with the 200-inch telescope. Those select few went on to measure the spectra and reap the headlines and the original discoverer left the field in disgust.

As a Carnegie astronomer I was observing on the telescope but the radio positions of the quasars were kept secret and so I did the next best thing - photographing peculiar and disrupted galaxies to see how they were formed and evolved. Ironically, in the end, they turned out to be surrounded by quasars which were obviously not out at the edge of the universe. That news was not welcomed by the observers who had inflated their reputations with discoveries of a new "most luminous object in the universe" every few weeks.

There followed an interregnum of about 17 years in which the Cal Tech astronomy Department pressed for a larger and larger share of the telescope time. One must know that in the operating agreement for the

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Observatory that the Carnegie astronomers were appointed full faculty members at Cal Tech. Then in 1980 Cal Tech broke the agreement, taking over the 200-inch and severing the faculty appointments of the Carnegie astronomers. There were bitter protests by the suddenly discharged faculty (Appeals to the American Association of University Professors were not heeded). In the subsequent allocation committees Cal Tech included only a few of the less senior Carnegie staff who then received small amounts of time but more time than the senior Carnegie members whose time was cut to nil. Telescope time was, and is, the currency of the realm, and in the competition for scientific preeminence the senior Cal Tech Faculty had just helped themselves to large bonus from the company assets.

But it is not just a question of territorial expansion and control, there is also the question of eminence and prestige and the impossibility of being wrong. This becomes clearer to me now when look back at the events of 1982-83. At that time I received a letter from the joint, Carnegie Institution of Washington - California Institute of Technology, telescope time allocation committee. It was unsigned but it said that if I did not give up my present line of research they would not allocate me any further telescope time. I responded with data showing my publications and citations far exceeded those of the committee members as well as other senior Cal Tech astronomers. But the following year Cal Tech had taken over 75 percent of the 200-inch time. Next year my time was reduced to zero. I resigned my supposedly tenured position.

This is how the elite body of astronomers, which is now the reigning authority in Astronomy, was formed. By now, of course, the students of Cal Tech have gone on to many other elite faculties and astronomers from Harvard, Princeton, Cambridge, etc. have arrived in Pasadena. So as with many self selected elites, their power has grown to be almost monolithic.

But why were they so intent on suppressing the small amount of observation time which tested the current paradigms? I must describe at this point a few of the observations which are so threatening. I think some specific cases can make it clear that the current paradigm is fundamentally incorrect. It will also become clear that the longer the contradictory information is suppressed the greater the catastrophe modern science will suffer.

Examples of intrinsic redshifts and non Big Bang cosmology

There are many crucial pieces of evidence I could cite but I will single out only three here as examples of the many similar kind of results which by now, with great difficulty, have managed to be published.

a) NGC 7603

Number 92 in my Atlas of Peculiar galaxies has a large companion on the end of a luminous arm. In 1971 a spectrum revealed that this companion was 8,000 km/sec higher redshift than the central, active Seyfert galaxy. This amount of excess redshift cannot be accommodated in the conventional picture where redshifts mean velocities in an expanding universe. They could not be at such different distances and be physically interacting. When Fred Hoyle heard about this he came up from the Cal Tech campus to my

Carnegie office and asked to see the original picture. In 1972 he gave the prestigious Russell Lecture at the Seattle meeting of the American Astronomical Society and outlined a theory whereby younger galaxies radiated intrinsically redshifted photons. His theory of growing particle masses was a more general solution to the conventional field equations but was physically a Machian (not Einsteinian theory). At the end of the lecture he said the NGC 7603 observation created a crisis in physics and we needed to cross over the bridge to a radically more general physics.

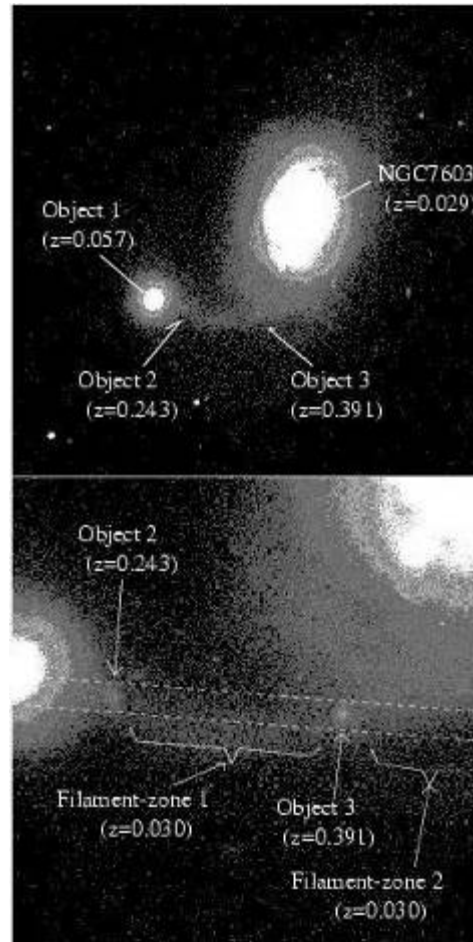


Figure 1. NGC 7603 is a Seyfert galaxy of redshift 8,700 km/sec. The companion attached to the arm has a red shift of 17,100 km/sec. Two quasar like objects of 72,900 and 117,300 km/sec have been discovered in this arm by López-Corredoira and Gutiérrez.

Over the years the evidence for non-velocity redshifts has grown enormously, both for quasars and galaxies. A number of researchers have tried to make the establishment admit the consequences of this evidence. But it has been suppressed and ignored. *However, In an event of great irony, 30 years after Hoyle's talk featuring NGC 7603, two young Spanish astronomers have announced the finding of two quasar-like, much higher redshift objects imbedded in the arm which connects the low redshift galaxy to the higher redshift companion of NGC 7603.* As in many past cases, this result alone should have settled instantly and finally the existence of intrinsic redshifts. Instead the paper was turned down by “Nature” Magazine, rejected

by the “Astrophysical Journal” and only finally accepted by the European Journal “Astronomy and Astrophysics”.

b) The Virgo Cluster

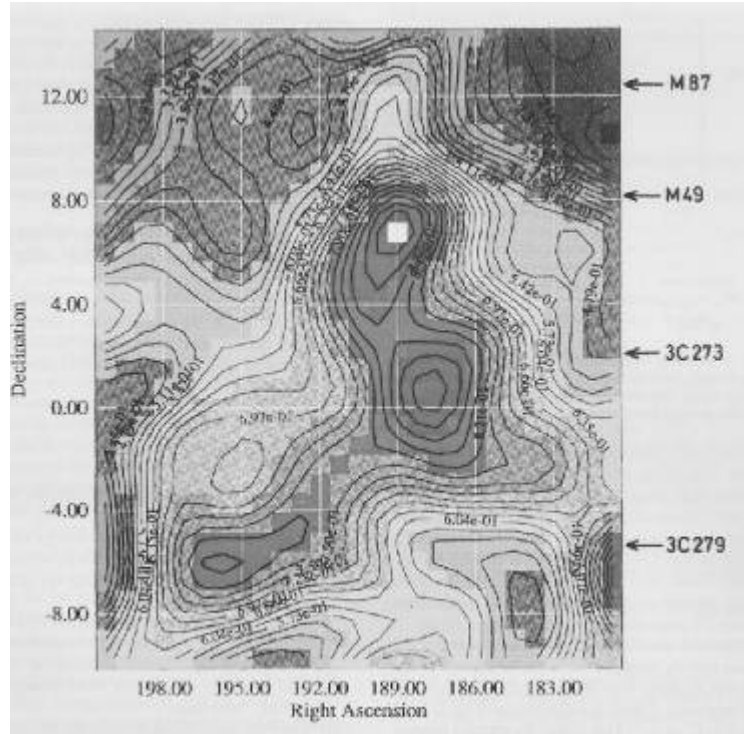


Figure 2. Gamma rays, greater than 100 MeV showing connection from M49 ($z = .003$) to the quasars 3C273 ($z = .158$) and 3C279 ($z = .538$).

In another case, the brightest quasar in the sky (3C273) was found in 1966 to be paired with one of the brightest radio galaxies in the sky (3C274) across the brightest galaxy in our Local Super Cluster. The chances were a million to one that they belonged to the Local Supercluster and that quasars were not at their redshift distances. Then this region was measured in high energy X-rays and the connection from the central low red shift galaxy to the quasar 3C273 was explicitly visible. The influential journal Nature refused to publish it although they had just published the top half of the X-ray map of the cluster. Then the gamma ray satellite came along and showed the cluster in the highest possible energy range, greater than 100 MeV. Not only was the 3C273 quasar at redshift = .158 attached to the central galaxy at $z = .003$ but the famous quasar 3C279 at $z = .538$ was also part of this high energy filament. The data was interpreted by Arp, Narlikar and Radecke as showing birth of new matter and new galaxies and the evolution of redshift from high values to low. It was published finally in Astroparticle Physics vol 6, 1997. The clear pictorial connection has been suppressed ever since and the original author of this extraordinarily important result is no longer a professional researcher.

The above is another kind of failure of the scientific system, unfortunately more common today. The orbiting observatory had been built at great expense, reduction procedures financed, and analytical

personnell salaried. When a great discovery was made it was hidden, not shown in conferences or published, because, for one reason, I believe, the team feared that they would be attacked as incompetent observers.

Some of the orbiting instruments that made epochal breakthroughs published results but ignored their significance. I visited one director regularly pointing out the obvious discoveries. He politely nodded and then went about ignoring the crowning achievement of his project.

c) The radio quasar 3C343.1

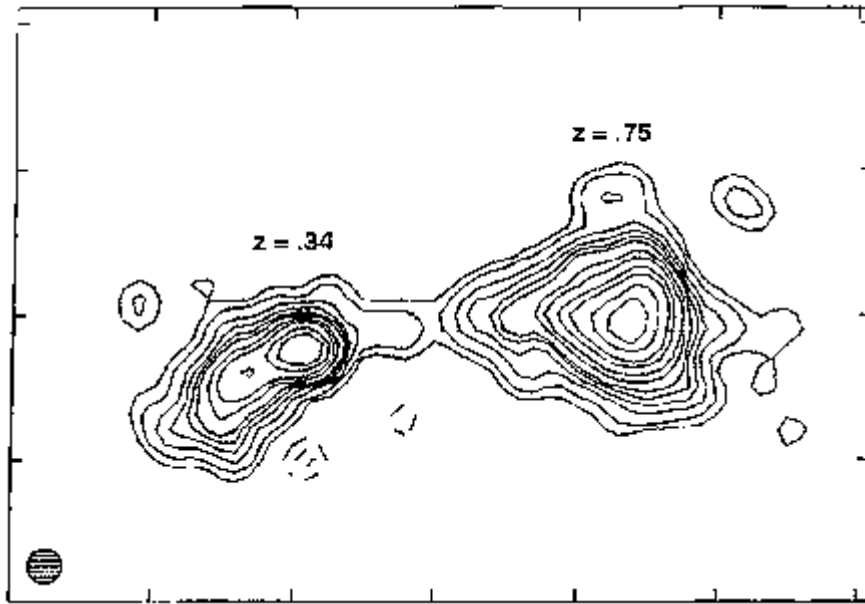


Figure 3. Radio map at 1.6Ghz of 3C343.1 by Fanti et al. A&A 143, 292, 1985. Separation of sources about 0.25 arcsec.

Science is based on repeatable observations of real objects and the relationships between them. In order to avoid generalizations, however, we show here another specific object which demonstrates the foundation of current extragalactic astronomy and cosmology is fundamentally, inescapably, incorrect. Fig. 3 shows a radio map of a strong radio source. Two redshifts are measured for this object with one much larger than the other. According to conventional cosmology they are in different parts of the universe. But we see they are, in fact, joined by a bridge of radio material. The chance of this observed configuration being an accident is one part in one hundred thousand billion! Other examples like this have been observed where the chance of accidental occurrence is only one in a billion. But this would seem to be the ultimate *experimentum crucis*.

The ejection in opposite directions of material from active galaxies, including very high redshift material like quasars, has been building up now for over 37 years. Yet the radio map shown here and the notation that his object had “two redshifts”, one a “background object”, lay unnoticed and unchallenged in the voluminous literature for 4 years! When it was finally submitted to the Astronomical Society of the Pacific it was rejected. In spite of my being a past President of this organization they refused other observational results and communications and I had to resign. It is particularly vexing that the A.S.P. has as a primary goal

educating the public about astronomy. But since it was hijacked by fanatical Big Bang adherents, it has been exactly misinforming the public.

We might also mention in passing that if the quasar redshift is transformed to the rest frame of the galaxy that it becomes $z = .31$, very close to the redshift $z = .34$ of the galaxy and to the quantized redshift peak of $z = .30$. Evidence has also been piling up for redshift periodicity 36 years—a result which is an instant refutation of conventional expanding universe theories. From time to time incorrect papers claiming to refute quantization of redshifts are published and papers demonstrating it are rejected.

Can Academia reform?

Since this enormous amount of contradictory empirical evidence has not been accepted over the last generation I personally believe that it will not be accepted until there is a fundamental change in the structure of academia. To start with routine operations, electronic communication today make it not sensible to pay for wasteful transportation of observers to remote sites in the world. Buttons can be pushed as easily in the home office. Observations could be performed by email request with small key observations having priority over larger, more critically reviewed programs. Countless conferences in exotic places of the world between mutually agreeing researchers tend to be vacation treats for the elite and their helpers.

Certainly Academic Science is overfunded in terms of the usefulness of their current end product. If more of this money were channelled instead to non-academic researchers there would ensue a pressure for the academics to consider seriously some of the more innovative and realistic work of people who were primarily interested in understanding their subject. Of course a more democratic science would introduce a lot of wild ideas but then research only by the elite seems to produce only bandwagon ideas which are sure to be wrong.

The only alternative to censorship (a.k.a. refereeing) in professional journals is personal communication between individuals and groups. Recently that has taken a great step forward with the internet. In any case, the professional academic journals will soon be expanding their shelf space faster than the speed of light. That will not break any physical limit because there will be no information involved (like cosmic inflation theory). But for the life blood of science, which is communication, there appears to be no hope in the public media which at present appears sound asleep.

The media

When a newspaper like the N.Y. Times hears about an event of international interest they call up the Whitehouse and ask the President what it means. That is featured on the front pages and perhaps a few Republican and Democratic Senators, and “think tanks” are quoted on following pages. Letters to editors and columnists with “respectable” views are reported further inside. Deep inside the Sunday Times, which hits the apartment door with a sound like thunder, can be found scraps of opinions by foreigners, artists and

miscellaneous people. Very democratic, you say, with opinions being represented roughly in proportion to their numbers in the society.

Not so. The Bush Republican's stole the 2000 election by stopping recounts in Florida, disfranchising thousands upon thousands of democratic voters, and finalizing it all with a right wing coup in the Supreme Court. The Times together with a few other "respectable newspapers" thought it over for a long while and finally issued a lame opinion that "Bush would have won anyway"—hail the chief! Aside from the loser being awarded the winner, no one mentioned that if the U.S. had the more representative democratic structure of many European nations, that they would today be governed by a democrat (plurality) -green, coalition of Gore and Nader.

The bad news is that the Times is the very best. The rest of the newspapers, the entirety of the TV and huge amounts of radio programming is given over to the most shallow repetition of what is believed to be patriotic slants of the news. Is it any wonder that most of the rest of the world was against premeditated war while the U.S. was reported to be 70 percent in favor? (Actually in Bay Area San Francisco, and other more enlightened communities, the sentiment was clearly reversed).

But now what happens when a scientific event occurs? The N.Y. Times calls up Princeton and asks their opinion. The professor tells them, "That report of a new observation has been shown to have been false. Everyone agrees that my theory is the correct one." If the Science reporter really gets serious he calls up Harvard, Cal Tech or Univ. of Chicago. He gets the same story that "Contradictory observations are incorrect and that the real controversy is over whether the undetectable 'dark' matter in the universe is 90 percent like I say, or 95 percent like some other prestigious scientists claim." The rest of the national media, understandably, do not mention it. Occasionally they run a story "Einstein invented dark matter and space is curved!"

Real investigative reporting is truly a lost art. In science it is horrific, with reporters never lifting their feet off their desk or their hand off the telephone. In politics, which people believe is more important, however, there are a few brilliant exceptions which show what can be accomplished with hard work. Two I would mention are Michael Moore and Greg Palast. (See internet for biographies and books published). They actually get the original records and confront the "experts" with what they have said and enumerate the statistics and facts which contradict the establishment consensus. And of course there is Noam Chomsky who is the leading founder of linguistics and speaks brutal truths for anyone who cares to consult his political writings.

How does reporting of astronomy and cosmology to the public compare with political reporting? What are the factors which control this science and do the kind of democracy which exists in western nations today control scientific knowledge?

Democracy and the media

The inescapable fact about western democracy is that it is heavily controlled by money. We all know that money buys political influence for the people who invest in public relations and lobbying. This influence in

turn leads to the more monetary return which can be used to gain more influence. In Science it is rather direct with Institutions and researchers applying to the government for grants and support. In politics one must influence legislation. But a public relations department is crucial for the image and most academic institutions have one. This activity is usually conflated with “educating the public.” One can try to limit funding contributions to politicians but it will be difficult to limit the euphemistic term “public education”. Perhaps we could try under the motto of “separation of church and state”.

The countervailing force of investigative journalism is difficult to encourage because it is so easy to just accept predigested hand outs from respectable sources. One must fall back on old fashioned democratic populism. The wide and wild opinion forum of the internet; the Meta Research Bulletin by Tom Van Flandern; books published by small publishers like Apeiron. Two books have now been written compiling all the discordant evidence; *Quasars, Redshifts and Controversies* and *Seeing Red: Redshifts, Cosmology and Academic Science*. Presently a “Catalogue of Discordant Redshift Associations” is published at Apeiron, Montreal. “A Different Approach to Cosmology” by Hoyle, Burbidge and Narlikar and all the references therein is available.

It is possible that long lasting changes must grow from the grass roots upward and that independent decisions by enough citizens will force the media to discharge its responsibilities and ultimately help redirect money into more productive channels.

Problems with directors, chairpersons and CEO's

Aside from Engineering and Medical Faculties which generally have to produce something that works, Academic Directors tend to be crippled with problems of power, prestige, cronyism and issuing degrees only to students who demonstrate that they know the correct answers in the subjects they have studied. The best results I have seen are in Departments who rotate the then onerous job of chairperson every one to two years. Diversity of independent faculty - while faculty still remains a working concept—seems best suited to achieve balance of power and interests.

Business is no less ruthlessly competitive and ethically challenged. Excessive executive compensation just welds seamlessly the connection between money and prestige. One overpaid entrepreneur was known to remark “Money is just a way of keeping score”. In a capitalist economy stockholders seem to be the only hope. They are beginning to realize executives most interested in money for themselves are not usually most interested in the health of the company or the world. In the very long run it may be that unregulated capitalism produces an exploitative evolution for humanity that is self limiting in that it destroys its own environment. A more adaptive type evolution may be slower but safer.

I might make a few summary remarks: Why has all the observational evidence been disregarded when it falsifies almost everything that is supposedly known about extragalactic astronomy? Perhaps the informal saying, “To make extraordinary changes one requires extraordinary evidence” really means, “To make personally disadvantageous changes no evidence is extraordinary enough”. I felt it was necessary to resign because freedom of research was the the most important issue and here was a rare factual issue that should

have strong reformatory effect when it turned out to have been improperly suppressed. As a relief from the disastrously competitive climate in the U.S. I found more tolerance in Europe. And the opportunity to change to X-rays, a different observational wavelength furnished new kinds of data and stimulation .

The beliefs of society

But finally, in the long view, is improvement in the moral basis of society necessary to bring about beneficial changes in both Science and Democracy? By moral I mean an operational definition of “that which will promote long term survival”. One of the problems is that we have a culture that rewards conformity more than innovation. Children are generally taught that there is always one correct answer. Not to get that answer means failure. That produces fear. One can see the effects in classes where the students do not ask questions (as in the graduate classes I taught at Cal Tech). One can see the effect persisting in mature scientists.

Education tends toward social indoctrination. The most important task of a school is not to teach *what* to think but *how* to think. Grades should also depend on questions asked as well as answered. The value of experiments, empirical versus theoretical analysis and testing fundamental assumptions should be emphasized. For many people this would mean liberal schools and elements of home education.

On the psychological and philosophical front one can ask questions like: “Why do people seek power? What can be done to make society and media less exploitative. How best to promote tolerance for divergent views and respect for nature. In the media, can we combat the unbearable hypocrisy surrounding military aggression?”

In a democracy scientific truth should not to be voted on by a self selected elite. I remember Linus Pauling, a double Nobel Prize winner, who nevertheless had trouble defending his professorship at Cal Tech, enunciating his Golden Rule: - “Do unto others 10 percent better than they do unto you (10 percent to allow for subjective judgement).” Perhaps then we may permit the race to evolve in the direction of what we call intelligence.

Peer Pressure and Paradigms¹

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Abstract: We critique the theory of the Scientific Method itself. We begin with the concept of “scientific peer pressure” and how it interferes with the objective examination of extraordinary ideas on their merits. We argue that extraordinary ideas should be more freely examined if science is to make progress, and we critique Occam's Razor. We examine how science deals with threatened paradigm changes. This leads to the formulation of “catastrophe theory” for the advancement of knowledge. We conclude with a discussion of the Scientific Method and scientific arrogance, and look at some major scientific conclusions in that light.

A note about scientific peer pressure

In my field of astronomy, why have I arrived at models so different from the existing conventional models? I disagree fundamentally with one popular tenet of the Scientific Method, “extraordinary hypotheses require extraordinary proofs.” Sometimes (although admittedly not often) the extraordinary hypothesis will be the correct one. If we continue to apply “Occam's Razor” and eliminate extraordinary hypotheses when ordinary ones can be made to do, this is fine as long as we do not strain plausibility. But in modern science, this preference for ordinary hypotheses over extraordinary ones is continually used to ignore plausibility, and to not give serious consideration to the merits of extraordinary ideas.

We have also seen throughout my book (see footnote 1) that the present system, in which extraordinary hypotheses are excluded without extraordinary proof, is still very much fraught with error, even in the fundamentals in many fields of knowledge. Very often we find the situation that a mediocre hypothesis has been adopted as “fact” on no better grounds than that no better hypothesis has come along for many years. I am convinced that admitting extraordinary hypotheses tentatively when circumstances justify it would advance science in two distinct ways: (1) Some of those hypotheses are true; and because they are extraordinary, they are unusually important. Even the effort of disproving them enriches the body of scientific knowledge. (2) By admitting hypotheses with less claim to certainty than usually required, all

¹ Reproduction of chapter XXI of the book “Dark Matter, Missing Planets and New Comets”, North Atlantic Books, Berkeley (1993; 2nd ed. 1999).

scientists will become more aware of the fallibility of all hypotheses, and perhaps be less resistant to giving up on those bad hypotheses they have already accepted.

As readers of this book may come to appreciate, the human problem with extraordinary ideas is that they often undermine the fundamental assumptions upon which so much other research work has been based. If an extraordinary idea with merit comes along, there is usually only one or a few people promoting the new concept, whereas there are many who have read, experimented, taught, published, and invested funds, time, and energy in consequences of the old theories which would need to be revised, if they are not outright rendered obsolete.

It is not so much that these individuals are intellectually dishonest as that they take the path of least resistance. Most of the time they lack the background or cannot invest the time to determine for themselves the merits of claims and counter-claims; so because of vested interests, they become “rooters” that one of their peers will shoot down the new hypothesis. They often leap to the defense of an esteemed colleague whose reputation would be “blemished” by the acceptance of the new idea—a thinly disguised excuse for failing to be scientifically objective. Most of this “rooting” takes place quietly: by ridicule in private discussions, in peer reviews by referees and editors, in decisions on grants by funding officials (who may have previously put millions of dollars into what would now be “obsolete” research).

What happens if a scientist has enough influence to get to speak in front of a large body of his peers about an extraordinary idea? This happened to me at an International Astronomical Union Colloquium in Lyon, France, in 1976, where I first spoke to my peers about the exploding planet hypothesis. I had widely circulated lengthy preprints for comment prior to that talk. Unbeknownst to me, a number of colleagues arranged with the meeting chairperson for three specialists to be called on in the discussion period after my talk to give prepared rebuttal remarks². Afterwards, the chairperson tried to cut off further discussion, although dozens of additional attendees still wished to ask questions or make comments. So one prominent specialist stood up and declared, “Based on what we have just heard, this paper is surely without merit and can be dismissed!” The response was emotional applause and cheers (without precedent for that scientific body) and the immediate adjournment of the session, postponing the remaining scheduled presentations. So much for the pretense of objectivity!

The term for what we are describing here is “peer pressure”. We have come to see how effective peer pressure is in getting people to try alcohol, cigarettes, drugs, coffee, etc.—things with deleterious health effects not in their own interests to try. But peer pressure operates throughout life, not just among the young³; and it is prevalent among scientists, who would even mount an international boycott of a publisher of a radical hypothesis (Velikovsky's) rather than address its scientific merits (which didn't happen for a quarter of a century, because of peer pressure). *The ridicule of scientists who present extraordinary hypotheses* is

² An example of the “sneak attack”.

³ To see its effects, consider cases of peer pressure in which almost everyone participates. Suppose someone chose to defy social conventions about clothing, hair-style, or public behavior in some non-intrusive way. They might be shunned and avoided, ridiculed, scorned, perhaps even threatened, in proportion to the degree of their defiance. Much of this will be done by people who believe they are acting in the defiant one's best interests. Everyday peer pressure from one's family, friends, and acquaintances may differ in degree, but not in kind, from this example. That is why it is so devastatingly effective in acting on all of us. The unified withholding of communication is always a powerful motivator!

scientific peer pressure in its most blatant form. It usually accomplishes its intended goal—to prevent otherwise neutral scientists from being willing to speak out publicly with any favorable remark about such an idea.

This environment in which scientifically counter-productive peer pressure flourishes is created and encouraged by modern society. It probably stems from our basically religious, and therefore authority-oriented, heritage. It also arises from the huge numbers of people who either are, or have been trained to think they are, judgment deficient, and who therefore rely on “experts” and “authorities” to form their judgments for them. Finally, this counter-productive climate is fostered by society's resistance to change, especially rapid or radical change. It is literally true that many people *do not wish to know* that reality differs from their present conception of it. Such people will always be a source of inertia to change, and an inhibitor to those of us who do wish to know. We might accommodate them by describing even the most certain of new models as tentative, allowing them to maintain their illusions. It should not be forgotten that there are some people who truly do not have the personal resources to cope with drastic change.

As for my own ideas, I have attempted to provide some testable implications of each hypothesis. The chief value of any hypothesis, extraordinary or not, is in its ability to predict things not yet known or understood. I would be content to have that criterion measure the value of my own ideas. But only readers such as yourself can help ensure they will be afforded the opportunity to be judged objectively in that way, rather than treated with unscientific methodology.

Paradigm change

Once a paradigm (a standard model with far-reaching implications) becomes widely accepted, most scientists acquire incentives to keep it in place as is. In addition to all those incentives, modern knowledge has become so vast and accumulated so quickly that it is becoming increasingly difficult, approaching impossible, for a single person to challenge a paradigm. This is most unfortunate for progress, since the motivation to challenge a paradigm usually originates from a single person.

Consider that in order to discuss and defend the exploded planet hypothesis among my colleagues, it was not sufficient that I simply put forward the evidence I found within my own field of special expertise, dynamical astronomy. I had to become familiar with dozens of sub-disciplines, many of them in areas so remote from my training that I initially had no knowledge or experience in them. Yet I had to become familiar enough with each subject to make comparative tests of how well the standard and alternative theories fit the data in each. To emphasize the point, since it is my intent to suggest an explanation about why so few scientists can successfully criticize the fundamentals within their own fields, here is the list of sub-disciplines I had to study to address just the exploded planet hypothesis:

The history of the conflict over the past 200 years, especially Olbers, Lagrange, Newcombe, Brown & Patterson, Carter & Kennedy, and Ovenden; the Titius-Bode law of planetary spacing; Ovenden's alternative “least-interaction action” hypothesis; dynamics and spatial distributions of asteroid orbits; collision theory and probabilities; explosion signatures among artificial Earth satellite orbits; Kirkwood gaps; gravitational

spheres of influence; tidal forces for small bodies; observations of asteroidal secondaries from occultations and radar; resonance theory; meteor streams; properties of meteors entering the Earth's atmosphere; bands of zodiacal dust; asteroid families; the Oort cloud; the “inner core” theory; the “Kuiper Belt” theory; the solar nebula hypothesis; theories of comet origins; dynamics of passing stars; galactic tidal forces; giant molecular clouds; physical properties of comets, such as formation and brightness of coma and tails; chemical abundances; statistics of orbital elements; correlations of physical and orbital properties; spectra and albedos at all wavelengths; lightcurves; Sun-grazing comets; comet splits and the velocities of fragments; tensile strengths of materials; non-gravitational forces; coma dust and OH bursts; spacecraft encounters; infrared trails in comet orbits; jets; solar magnetic sector boundaries; distribution of dark material throughout the solar system; cratering distribution and statistics; meteorite types, physical and chemical properties; theories of meteoroid origin; geology and climate of the Earth; “mascons” on the Moon; librations; magnetic and radiogenic character of all extraterrestrial materials; erosion rates; flow properties of water on various surfaces; capture and escape mechanisms; “spots” in outer planet atmospheres; accretion; rings; lenticular cloud formation; hurricanes; cosmic ray exposure ages; fission tracks; chemical differentiation; cooling rates; shock and pressure effects; isotopic anomalies; carbon isotope formation and diamonds; fireballs; interstellar hydrogen clouds; X-ray and extreme ultraviolet backgrounds; gamma-ray bursts; matter-antimatter interactions.

It is my opinion that when so much evidence for a hypothesis is brought forward, either the hypothesis deserves a place on the scientific table for further evaluation, or other scientists acquire an obligation to show cause why it should not. “Showing cause” implies open discussion of the merits, not unilateral declarations of error.

The value of extraordinary hypotheses

Some of the ideas set forth in the chapter “The Origins of the Solar System and of Man” of my book *“Dark Matter, Missing Planets and New Comets”* are sheer intellectual speculation. Yet I feel certain there is nothing wrong with them as viable hypotheses about our origins, and that they are entitled to the same scrutiny from the Scientific Method as other, more conventional theories of origins. It is a perfect example of why the axiom “extraordinary hypotheses require extraordinary proofs” is harmful to scientific advancement: since the extraordinary hypotheses are, by selection effect, necessarily those we least suspect to be true, it follows that they are the ones with the least surviving evidence⁴. We shall be condemned never to discover the most extraordinary truths unless we are prepared to make exceptions to the rigorous application of the “extraordinary proofs” criterion.

This approach does imply a willingness to accept many more hypotheses for investigation than one otherwise would do, in the full realization that most of them will be false. The point of doing so is that we may expect the occasional true extraordinary hypothesis will be of considerably greater importance to the

⁴ Extraordinary hypotheses discovered to be true become accepted into conventional science and are then no longer extraordinary.

advancement of knowledge than most ordinary hypotheses. A further aspect of our more liberal approach is a tolerance of ambiguity. We admit that knowledge is often tentative and incomplete, and that all our accepted hypotheses have a certain probability of correctness attached to them. We are here expressing the desirability of lowering the required probability of correctness before accepting a hypothesis for serious consideration. Although we may take a higher risk of accepting false hypotheses into our body of knowledge by so doing, we may also include a greater number of the most important true hypotheses for which proofs are not presently possible, or may never be possible, because of data destruction. The origin of the human race is a hypothesis of this sort, since hard data is virtually nonexistent.

In a perverse way which frustrates conservative scientists, the preceding premises will alter the balance of proof fundamentally in numerous cases of research on the frontiers of science, where inadequate data exists. This is because a cataclysm or other extraordinary event will often suffice to explain a multitude of phenomena, all of which would otherwise require individual causes and explanations. This is sometimes referred to as “catastrophe theory”.

The case in point is the origin of the human race. By either Von Daniken's approach or by Sitchin's, Occam's Razor argues that the single hypothesis of earlier contact with extraterrestrials to explain the wonders of the ancient world and the remarkable agreement among ancient texts in speaking of visitations by “the gods” should be preferred to the multitude of separate and ad hoc explanations others have offered. If mainstream science were not so preoccupied with avoiding extraordinary hypotheses, it would surely be agreed by most parties that the evidence, severely lacking though it is, mildly favors the extraterrestrial visitation hypothesis over most others. However, it cannot be argued that the evidence is anything approaching compelling, especially since it is all indirect (i.e., no definite extraterrestrial artifacts have been found). And since the hypothesis is certainly extraordinary, science prefers to reject it until and unless some extraordinary proof comes along.

But what if the hypothesis were true, but most of the evidence has been destroyed? I wish to argue for the legitimacy of accepting such hypotheses for serious consideration on the basis of preponderance of evidence, even though the idea is extraordinary and the evidence far from conclusive. In other words, I am arguing for the admittance of a body of “tentative” knowledge to mainstream science, labelled as such, which has some lower than usual probability of correctness attached to it, or some ambiguity of interpretation. And, I contend, the benefit of so doing will be a more rapid advance of science, because the truly significant breakthroughs will often be represented among these tentatively accepted hypotheses. We would not call such ideas “facts”, as we tend to do with conventional theories. Instead we would refer to them simply as hypotheses under investigation, having both supporting and contrary arguments. It does not threaten the integrity of science to state that among other hypotheses under consideration for the origin of man is the possible intervention of extraterrestrials. The pro and con arguments would make interesting reading for all. And the hypothesis does make testable predictions, some of which we examined in the chapter “The Origins of the Solar System and of Man” of my book *“Dark Matter...”*. Why should it be disreputable to engage in such a discussion?!

Catastrophe theory

The most elegant example I have encountered to illustrate catastrophe theory as applied to the advancement of knowledge (and by extension, to other areas as well) is based on a mathematical analogy first brought to my attention by S.V.M. Clube. Imagine that we are making observations of some phenomenon. When the phenomenon is first discovered, nothing is known about how it changes with time, so let's represent a quantitative measure of its magnitude by the constant 1. Later, a change is perceived as a function of some independent variable x (perhaps the time; let x be measured in units such that it is a fraction less than 1). We then represent the phenomenon by the expression $(1 + x)$, and make predictions of its future behavior from this "theory". Still later we detect some deviation from our predictions, which remains mysterious until one day a learned scientist discovers that the deviations can be represented by a quadratic term, and that the corrected "true" theory should be $(1 + x + x^2)$.

After much time elapses, new discordances with the observations (the existence of which are at first denied) become too great to ignore. Yet another learned person investigates the problem and discovers a cubic term. The theory $(1 + x + x^2 + x^3)$ now represents the observations so well that the validity of the new theory cannot be denied by "reasonable" men, and it soon replaces the old theory in all the textbooks. The process may continue indefinitely, perhaps spanning generations of learned researchers, each adding his own significant new refinement, as important in his time as was his predecessors'. The theory becomes ever so complex: $(1 + x + x^2 + x^3 + x^4 + x^5 + \dots)$. Though all students learn how to make correct calculations with the theory, only a few minds fully understand it.

Of course, it never succeeds in making correct predictions very far outside the domain of the observations used to formulate the theory.

Then one day, a radical scientist, reasoning from an entirely different perspective thought to be irrelevant, makes a startling suggestion: the real theory for the phenomenon is $[1 / (1 - x)]$. The new theory looks nothing like the old one and makes radically different predictions about the expected ultimate behavior of the phenomenon far from the domain of the observations, so it is not given serious attention. It may even be forgotten altogether and have to be rediscovered many times. One day, however, a compelling fact forces a confrontation: the radical new theory continues to represent the observations better than the old theory, however many terms are patched onto the old one. In fact, it passes the most important tests of any radical new hypothesis: it has correctly predicted the future when other theories failed, while adding insight and understanding of the phenomena.

Once this is realized, the entire body of science based on the old theory may be overthrown. The discoverer of each new power of x in the old theory is lowered in prestige by the revolution. Textbooks must be rewritten. Whole fields of investigation based on the old theory become obsolete. All the consequences of the new theory must be carefully redeveloped from first principles. Scientists are not at all pleased by these developments (except young, new ones, who have no vested interest in either the correctness of the old theory or in its application), but eventually, the last of the discontented "old-timers" dies off. Then it

becomes difficult to imagine a time when it wasn't obvious that such a well-known phenomenon can be predicted so accurately in such a simple way.

I refer to the discovery of each new term in the old theory as ordinary scientific progress. I call the discovery of the new theory a “revolution” or “catastrophe” of knowledge. Nothing about scientific progress demands that the revolution ever occur. Ordinary scientific progress may continue, quite literally, forever (through an infinite number of powers of x). But if a catastrophe of knowledge does occur, it makes obsolete most of what has gone on before it, however many “powers of x ” may have been added.

And so it is, I strongly suspect, with all human affairs, wherever evolution is operative.

Scientific arrogance

There is one last point of considerable importance to this whole topic of the advancement of knowledge and the issue of evolution versus revolution: I call it the *arrogance* issue.

Everyone who would espouse an extraordinary idea and take unilateral action based upon his own belief that what he does is good is acting arrogantly; and most of the time, the rest of us wish to be spared from the “good” such a person thrusts upon us. So when are such actions justifiable, if ever? Scientifically, is it ever justifiable for a man to conclude that he is correct and everyone else who doesn't agree with him is wrong?

Individuals can deceive themselves almost without limitation. But it is possible to “reality test” and tell the difference between self-deception and improbable reality. In fact I have mentioned cases in this book where the universal opinion of relevant authorities is almost certainly wrong. Now let me review one case where I am confident that I am correct, against the opinion of almost all authorities in a particular field, and why I think such a conclusion is supportable by objective criteria.

In brief, I maintain that there is only one reality; and to know it, one must objectively test reality. Such a test must distinguish the truth of the hypothesis one is testing from the condition that it is false; and both results must be *possible* results of the test—i.e., the hypothesis must be falsifiable. One might add the further conditions that in order for a hypothesis (whether true or false) to have value, it must aid understanding and be able to predict things not already known.

My example is the nature of comets. The first distinguishing characteristic of comets is the appearance of a “coma”, or bright area, usually accompanied by a tail, always pointing away from the Sun. Astronomers trying to explain comas and tails finally settled on Whipple's “icy conglomerate” model, in which frozen water and other volatiles are heated as they approached the Sun and driven off the nucleus. It was expected that comets therefore reflected most of the light that hit them from the Sun, as ice and frozen volatiles would do. But it was then discovered that the material of comets reflects very little light, being in fact darker than coal dust.

So the theory was amended to accommodate that observed fact, and comets became known as “dirty snowballs”. But then it became known that “new” comets (approaching close to the Sun for the first time) lost an order of magnitude in brightness in doing so; whereas comets making their second or later approach had no detectable further changes in brightness. The theory did not anticipate this development but could

accommodate it: comets must be dirty snowballs which underwent extensive surface melting on their first approach to the Sun, then froze into a hard “crust” as the comet receded, thereby preventing such extensive further losses on subsequent passes.

When it was observed that comets sometimes split, it was concluded that comets have very low internal strengths, are quite fluffy, and fall apart easily. Still more recently, comet expert Zdenek Sekanina, analyzing the way in which comets appear to yield so easily to the relatively gentle solar radiation pressure, undergoing significant non-gravitational accelerations away from the Sun, concluded that comets were more like dirty, “flat, fluffy pancakes!” When other observations suggested more normal densities, there was discussion of violent internal processes in comets, and strong outgassing jets were invoked.

All these conclusions were derived from the observational data and seemed compelling to the authors at the time they were proposed. Concerning the nature of comets, this is what I mean by $(1 + x + x^2 + x^3 + x^4 + x^5 + \dots)$ in the mathematical analogy: the theory keeps getting patched to accommodate new data, no matter how intrinsically clumsy or absurd the model becomes.

This illustrates the general hazard of reasoning inductively. There are sometimes many causes that can produce the same effect. By examining the effect, we are led to guess what seems the most probable cause. But if the effect happened to be produced by some other cause, we may be unable to reason backwards to derive that fact. It is an unpleasant reality that inductive reasoning from effect to cause will sometimes appear to be unique when in fact it is not at all so.

The intrinsic value of deductive theories

Forward reasoning, by contrast, is not subject to that hazard. Time behaves like a non-reversible dimension, and cause-effect relationships always proceed in forward time; hence deductions that proceed in the same direction as time are potentially unique.

I do not see how one could easily have reasoned inductively to the satellite model for the nature of comets from the observational data available. But when the idea that comets are multiple-body gravitationally bound systems proposed itself as a corollary of the “exploding planet” hypothesis⁵ and from the observations that minor planets have satellites, it became possible to derive the logical consequences which would follow from such a comet model using forward reasoning. Let us see how well these hypothetical consequences fit the data.

The gravitational sphere of influence of a comet, within which it holds satellites stably bound, increases in size as the comet recedes from the Sun, and decreases as the comet approaches the Sun. The gravitationally bound cluster of bodies, complete with gas and dust, would appear as a “coma” around a central dominant body, or “nucleus”. Solar radiation pressure would drive some of the gas and dust away from the Sun, forming a tail. But the material is not required to come from the surface of the nucleus; rather,

⁵ The value of any hypothesis must be judged by the predictions it makes. If the satellite model for comets is ultimately confirmed in its essentials, then the “exploding planet” hypothesis which predicted that model will have accrued additional credit.

it may already be orbiting the nucleus or residing on very small bodies in orbit, and can easily be pushed to the threshold of escape.

Since comets in this model are made of carbonaceous rock, just like the minor planets, we expect that comets will reflect very little of the light that hits them, just as for minor planets. The first time the comet comes near the Sun, its sphere of influence is forced to shrink in size to a new minimum, which in turn forces much orbiting coma material to undergo gravitational escape, leaving the vicinity of the nucleus forever. But on subsequent returns, the comet would have no further losses of material so extensive, since the minimum size reached by the sphere of influence would be essentially the same as that reached on the first approach. These natural descriptions of comets derived from the satellite model happen to also be just what we observe.

In the satellite model, satellites can escape from the nucleus, primarily through the action of tidal forces, but only while the sphere of influence is shrinking as the comet approaches the Sun. This is in agreement with observations of what are called “splits” of comets. In the satellite model, the entire surface area of the coma feels the effects of solar radiation pressure, so accelerations away from the Sun are relatively large, even though the densities of the constituent orbiting bodies are those appropriate to rock.

The essence of the new model is the presence of satellites of significant mass orbiting the nucleus of all comets and comprising the coma. The essence of the old model is that all the mass is concentrated in the primary nucleus, which is the source of the gas and dust in the coma.

This new satellite model is the $[1 / (1 - x)]$ in the mathematical analogy: a revolutionary way of looking at the same data that fits as just well but which makes drastically different predictions about the nature and evolution of comets. For example, this model predicts a relationship between velocity and solar distance for components of split comets that is quite different from the prediction of the dirty-snowball model in any of its current variations. In fact, the observations have shown that the prediction of the satellite model is correct, and that of the dirty-snowball model is wrong.

At this moment, I am not trying to argue again that the satellite model is correct and the dirty-snowball model wrong. I was discussing “reality testing” of ideas, using this as an example. The satellite model of comets makes a number of specific, a priori predictions about the nature of comets which happen to be an almost exact description of what is known about comets from observational data. None of these observed characteristics were predicted by the dirty-snowball model; instead, that model was patched after the fact to accommodate them. A specific prediction that could distinguish the two models was made. A failure of the prediction would have falsified the satellite model. The observational data strongly agreed with the satellite model prediction and disagreed with the dirty-snowball prediction. Moreover, the satellite model has already been successful at predicting other comet phenomena which were previously unknown.

I submit that these criteria, carefully and objectively applied, are both necessary and sufficient to conclude that an idea is correct and has merit, regardless of the strengths of opinions or numbers of authorities who oppose the idea. Naturally it is implied in the testing procedure that those authorities have also had a chance to consider the idea and to make their best arguments why it isn't so. If no substantive

counter-arguments appear, I submit that it is not at all *arrogant* to conclude that one's own reasoning is correct and almost everyone else's is incorrect; rather, it is logically compelling.

The chapter "A Synthesis of Evidence for a Recent Planetary Breakup" of the book "*Dark Matter,...*" provides yet another example. With so much evidence favoring the breakup theory, I am not at all embarrassed to advocate it, despite the almost universal opposition it has received from colleagues, largely because it is an extraordinary hypothesis. Of course, it is not quantity of evidence that counts, but quality; but on that point also, the evidence is excellent. Looking at that evidence as objectively as possible, I have simply concluded that it is more probable that the judgment of most of the world's experts is wrong, than that I have been deceived by the evidence. Each reader, including those same world experts, must of course make that judgment anew for him- or herself after reading the book.

Having the courage of one's own convictions is the opposite of succumbing to peer pressure. Someone who appreciates the fallibility of human reasoning but nonetheless applies the Scientific Method with brutal objectivity and lets the results speak for themselves has nothing to fear from the judgments of peers, or of history. Only those who fail to recognize reality for what it is, and instead try to tell reality what it must be, need fear that judgment.

We have all experienced at one time or another an inspiration or a flash of insight. A scientist regularly comes up with inspired ideas in connection with whatever problem he is working on. He carefully examines each of these against the data or an experiment and determines which ideas work out and which must be discarded. Almost all scientists have learned to live with the disappointment that follows when a particularly excellent idea proves unworkable.

But they also know the joy of seeing an idea work well, even beyond the scope of what it was supposed to do. Such joy may be experienced when one comes upon a good starting point for a deductive model. By its nature, the deductive theory usually has little or no latitude. Its conclusions are predetermined by the starting point and the rules of logic. When such a model has a motivation other than the observations, and predicts and explains all the significant independent facts in a logically compelling way with no loose ends, and provides a new, simpler understanding of a phenomenon than anyone has previously elaborated, and when discussion before experts turns up no fatal flaws of logic or oversights of fact, one may hold to and use the model with a confidence that cannot compare with the assurances of numerous world experts that some other model is "the correct one".

The "exploded planet" hypothesis was such a model for me. When I read of the idea in Ovenden's papers, I was certain that applying it deductively would soon lead to contradictions with observations, as deductions from a bad starting point invariably do. But one of its first deductions was a new, plausible explanation for the origin of the logically absurd "Oort cloud" of comets. It made so much more sense than the conventional theory that I immediately knew the model had some merit. But then the model made prediction after successful prediction, offering new explanations for so many disparate phenomena throughout the solar system. It eventually became inconceivable to me that the model could be fundamentally wrong, no matter how many colleagues withheld their approval even after their objections were answered.

The promise of the extended Scientific Method recommended here is that we are more likely to arrive at true descriptions of reality by accepting tentative hypotheses and using deductive reasoning than by polling the authorities on these subjects. That is itself a theory to be verified, but with the potential rewards so great, no time should be lost in experimenting with it!

I conclude with a question that is a challenge to every reader, and to every person who seeks the advancement of human wisdom. In his five books discussing literal translations of the most ancient surviving texts, Sitchin⁶ discusses how the extraterrestrial “gods” who engineered the human race as slave labor became alarmed at the imitateness and inventiveness of humans. One of them is alleged to have bemoaned that nothing seemed beyond the capabilities of the humans, and that if left unchecked, someday the distinction between the “gods” and the humans would disappear. (The Bible story of the “Tower of Babel” is a conventional translation of later renderings of the same texts). To prevent this merging, the superior beings planned deliberately to interfere with the advancement of the human race. One of the steps they supposedly took was the partitioning of the species into segregated groups dispersed over the continents, each with a different language to inhibit communication among them.

Just now, at the end of the twentieth century A.D., humans have made serious strides in finally overcoming the handicap of partitioning by continents and languages, however that circumstance came about. Advanced communications and networking are accelerating the pace of discovery and advancement globally. It might be argued that the largest remaining impediments to progress are the presence of false hypotheses within our collective body of knowledge, and the resistance to change on the part of many humans.

The presence of false extraordinary hypotheses is a problem exacerbated by the overly credulous. Such people have little influence on others, but the bad name they give to extraordinary hypotheses is harmful. At the other extreme, most of us have experienced the frustration of dealing with humans who resist knowledge, progress, new ideas, or change. I carefully distinguish this emotional resistance from healthy scientific skepticism, although the former often masquerades as the latter. The skeptic must ask himself, given reasonable evidence for an unlikely thesis, if this skepticism would disappear willingly, or if he will experience emotional resistance to its departure.

Consider the thesis that the tendency to engage in such counterproductive behavior (excessive skepticism and excessive credulity) was genetically inbred into the human race by the hypothetical “gods” to further slow human progress. An insidious idea, but potentially very, very effective! Suppose we learned that it was true. How would we respond?

Well, shouldn't we respond that way, anyway?!

⁶ Sitchin, Z. Five books available from Avon Books, New York: “The Twelfth Planet” (1976); “The Stairway to Heaven” (1980); “The Wars of Gods and Men” (1985); “The Lost Realms” (1990); “Genesis Revisited” (1990).

What do astrophysics and the world's oldest profession have in common?¹

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Dedicated to Eduardo Simonneau,
eye-opener master

Life is the best teacher, much better than university. I have learnt some things about astrophysics during my last ten years as researcher, but I found other things on the earth that were also worth learning. Trying to understand the mechanics of stars and galaxies is beautiful and I am glad to dedicate my time to this noble occupation. However, one should always bear in mind that we are on the earth, surrounded by other men, and in that respect we must be aware of the floor we stand on and not only look at the sky.

I do not think that astrophysics is a special case within the sciences. In the same manner, I do not think that science is a special case within the world of the administration of the culture. All of us are in the same ship: the world in the capitalist era. Nevertheless, I will concentrate on astrophysics as a human activity since I know this world better from inside (I also know the faculty of philosophy, another “casa de ...” which has more to be criticized about than the institution of astrophysics, but not here). In the present paper I want to tell of my impressions about this world, which I know from close observation, in an open way and without self-censorship. I want to tell the facts as I think they are, without worrying about whether this is nice for the reader or not, or whether this can be published somewhere. I think the only method of reaching the truth is to never be afraid to tell the truth, and to put it before other interests, such as prospering from the system, getting a position, publishing in prestigious journals, etc. Probably, there might be errors in what is said here, since they are my subjective appreciations. However, they do not matter when the goal is important: saying honestly what one thinks.

The way the actual institutions work is very complex. One begins to understand them once one has worked some time in them; inside them. I think that a philosopher or a sociologist cannot correctly infer a plausible theory on social mechanics in the sciences by reading four books, without any direct knowledge of what is being ‘cooked up’ in these institutions. Perhaps, it can be inferred by comparison with other institutions, but not from simply reading some books, since there are practically no publications that reflect the real situation. Truth is not always achievable through a bookworm, since some truths are not written or

¹ Put as preprint in the web page [arXiv.org](https://arxiv.org) with reference astro-ph/0310368 on October 14th, 2003.

their diffusion is very limited (as may be the case with this text), or perhaps because somebody has an interest in them remaining unknown.

Students

The first contact with research takes place when one prepares a PhD thesis. Here, as in many other disciplines, the system adopts a clear position: “things are as we say; either you take it or you leave it”. If one wants to work on the research, one must be in the service of a program that is predetermined by the authorities responsible for the system. If the student wants to get economic as well as department support, then his or her role must be obedient to, and assimilating of the traditions of the department.

In a jokingly ironic and cynical sense, and with a certain presumption not so very far from truth, it is usually referred to as student “slaves”. The slaves are in charge of doing the most monotonous research tasks (observing during long periods through the telescopes, data reducing, etc.) in the service of the team where they work. There also exist in the hierarchy of the system some inferior figures: the temporal (for a few months) scholarship holders; they are normally pupils who have not finished their career and, therefore, they are under PhD students. Those are usually called ‘summer slaves’, because their contracts are in force during summer months, and there is not enough time in such a period for them to learn something about research. Therefore, they are used as cheap manpower: a few days to learn a mechanical task and the rest of the summer to apply its routine.

This appraisal of the treatment toward students is really not always applicable. In my case, for instance, it was not. However, I feel certain that the reality of this exploitation reality is quite extensive and rather more common than would be desirable. Of course, I am telling what several researchers have told me rather than describing statistical data published by some official organization. Nevertheless, I judge that the sources are sufficiently representative.

In some cases, those students that do most of the work cannot write their results in a paper, but the bosses do it instead, as first authors of the paper. Students are told that they do not know how to write their own work. In other cases, when the supervisor sees that things do not have the outcome he or she wants, the supervisor abandons the student. In some cases, the supervisor steals the ideas of the student. In other cases, the granting of the PhD to a student is over before the thesis is finished because he or she was exploited by having to perform other activities aside from the writing of the thesis, or the boss had no time to attend to the explanations produced by the student. In such cases, the student must struggle to survive while finishing the work.

Few bosses sit down and work with students. Normally, they spend some time during the early days to explain how to do things. After that, the student must do the routine tasks. The boss just gives the ideas, if they have them; otherwise, just makes minor corrections. The student spends weeks or months in front of the computer, fighting with a program that does not work, with annoying calculations or simulations that consume a lot of time. Students spend complete nights at the telescope (the boss is usually present as well, but only the first time to explain to the pupil how the machine works, or when there is a novelty or

extraordinary observations unrelated with the usual routine. It is very normal that they go to sleep at midnight and leave students with routine work till the dawn if there is nothing holding them there), for complete weeks at the telescope. After that, he goes down with the tapes of several gigabytes of data, and reduces them; that is, processes them, and extracts information from the observations. This task usually requires several months. If there is some complication due to an error in the procedure, and the reduction must be repeated, it will take longer.

Meanwhile the boss manages and puts ideas. The boss will say: “All right, but you could do what’s his name, or that, or this thing beyond that”. The student will spend one week to do what’s his name. He will spend two weeks to do that and finally he will realize that it is not feasible. This thing beyond that is surely a stupidity but the boss cannot be convinced of that until it is checked with some calculations (of course, carried out by the student). Finally, he delivers the results to the boss, and he says: these are nice but I prefer it as they were before.

Which objection addresses this situation? Is it not normal by any chance that the master teaches the student, and the students effect what they are ordered to do while learning? Certainly, it must be so. The fact is that the recently graduated student does not know much about the specific area of work, and must be brought up to date. Nevertheless, they are not novices without knowledge. Usually, they have more general knowledge about astrophysics as a whole than the specialist who knows much but only about their own specific area. Moreover, students have some advantages over the master in this case: they are more creative, more open—with less prejudice—and can give new and fresh points of view about research to develop, instead of following anachronistic traditions that are embedded in the interests of the person who has spent a whole life with one idea. PhD students can produce ideas if allowed to produce them, even away from the track that was predetermined for them. In this regard, it would stand more to reason that the monotonous work should be in the hands of those with exhausted creativity, those aged, reputed experts who will produce nothing but copies of what they have always produced. However, the world of science does not stand to reason but that of power: the captain gives the orders to the sailor. Because of this, it is usual for routine work to be carried out by students. This is not so that they may learn (since one learns the first time, but not by doing the same task a hundred times), but in order that they produce. In some few cases, PhD students do their own research along with their duties with the supervisor, and in fields other than the subject of their thesis (I would recommend this to the future students), but that is not the most common way.

I was once told an anecdote with regard to this. I ignore whether it was true or not, but it seems that it is a real case: a student talks with his supervisor and says “I had an idea”. Then, the supervisor replies: “Ah! You have time to think?”. Is this the way to form future scientists? Having them spend time in a thousand routine tasks without free time to think freely? Thinking how to corroborate, yet again, with an idea that originated from a specialist of repute is to be encouraged. However, spending time to think about one’s own ideas, without permission, is something that is really not encouraged by the system, quite the opposite. Initiative is discouraged with arguments such as what is established is well established. Workers for science instead of thinkers are created.

What is the objection in this situation?—I continue to ask. Mainly, that creativity is not taught but industrial (mass-produced) science, and the period of optimum creativity of a scientist is exhausted with these ups and downs. We must take into account that, in the long history of science, the majority of great ideas were produced by young scientists. If young students, who could potentially produce new ideas, are used as slaves (or perhaps it is better to say 'workers for science'), then perpetuation of the ancient things and stagnation of intellect is rife. Even though, apparently, if one were to believe the mass media, a scientific revolution is being produced every day.

Postdocs, permanent positions

The researcher who wants to live on his work in the research world must aspire to a permanent position, that is to say, to become part of the body of functionaries, well known by all because of their efficiency at all levels. Beside jokes, the fact is that the life and motivations of the postdoctoral researcher are marked and oriented towards the obtaining of the position as the ultimate purpose. In the best of the cases, the functionary will still be motivated after the goal is obtained, but in many other cases the opposite thing happens.

A student who has just read the thesis obtains rarely an immediate permanent position, but he/she must before go through several institutions (some of them necessarily in a foreign country, although there are exceptions) with temporal contracts known as “postdocs”. I think really that this is one of the cleverest things the system has, because at least the researcher has some years of his/her life to look for their own pathways in research and, at the same time, avoid the early stagnation that is usually produced by the early permanent positions.

“Postdoc” status constitutes a hierarchical position of workers for science above the PhD student, but below the researcher in a permanent position. A large part of the routine scientific tasks falls to these figures but to a lesser extent than the PhD students since, in quite a lot of cases, they have their own mobility. In many other cases, they are contracted workers for a predetermined program. I think that travelling to a foreign country or not is of little relevance. Perhaps it is important only to become more fluent with English or other languages. Nowadays research is quite global in nature. Few things can be learnt in a country that cannot be learnt in one's own. In all places, the same thing is told and the same thing is done, with minor differences. The type of research to take on a contract has, perhaps, a larger influence than does travelling to a foreign country. Generally, after completion of a thesis, the researcher continues to be surrounded in the same type of environment, so he/she does not learn much that is new.

It must be said that not all doctors continue their careers as researchers. In many cases, motivations from private life are obstructions in the requirement for mobility in the job. Also the high competition necessary makes it possible for only a few to continue. In order to gain a postdoc position, a good curriculum is necessary, which is not necessarily associated with genius but the capacity to work, and the support or recommendation of somebody within the system. Without the appropriate recommendation, a career may be truncated. Hence, it is necessary to look for congeniality in the world. A way to look for congeniality is by

following the stream of general trends in research without trying to create a critical front. With regard to the curriculum, the major weight normally comes from the number of publications in professional journals. I say number (quantity) rather than quality because the predominant parameter is really the first one. Quality is valued when the committee evaluating the person who is applying for the position is a specialist in the same field and with the same ideas (that is, not a competence in defence of other theories). Since each specialist thinks that his/her field is the most relevant, the curriculum will always find support with respect to quality when it is oriented towards the interests of the judging tribunal. Otherwise, it will be just a number, evaluated with weight, as with school tasks.

This way of evaluating the efforts of a researcher will be a constant throughout the researcher's life; either to secure a position, or to obtain telescope time, or to fund money for a project, etc. Later, I will talk further about these questions. With regard to the postdoc positions, we must see in this evaluation system an indirect pressure over the putative free choice in research in order to focus towards those already given. The number of publications with little critical content, as well as short-term congeniality, is an indication of this pressure. These will be the factors used to secure other postdocs, or a permanent position, if the person does not leave the research before, or chooses to earn his/her living in another way.

In my experience, for instance, although not many crashes have taken place, I have been working in some non-orthodox fields and, consequently, I realized the problems that arise when one works in a field that was not recommended. Due to my approach to some researchers who work in areas of scientific theory with little orthodoxy in order to discuss certain data or exchange opinions, I had to listen to much advice. They claimed to have gone away from these research fields and all possible relationship with the aforementioned researchers. The rest of the community could relate to their position and this would be an obstacle in securing a postdoc or permanent position in the future. When I was invited to give a talk about the topic in a certain institute, a senior scientist said to me that giving such a talk would mean forgetting about obtaining any position there. This is not blackmail, but it is close.

From my own experiences and those of others, I have deduced that doors are opened and offers made to those who are servile and uncritical. A lot must be produced, but without great aspirations to say something important. It is sad to have to say that the positions I obtained were given thanks to the works I consider less relevant, while those works that I consider interesting have created problems for me, together with much discussions, headaches, and inattention. It is sad, but it is so. I am certain that I am not an isolated case.

Publications, referees

The fruits of scientific activities are gathered in specialized journals, those journals that will be read by other specialists and distributed in libraries of the research institutes all over the world, with exorbitant prices, either to publish or to receive the journal, which can only be afforded by the wealthy institutions. Of course, it should be mentioned here that the scientific journal business is not a trifling question. Nowadays, the journals are a powerful communication tool, written in the international language of English, and with enviable accessibility. It must be recognized that the present-day system of scientific publications is very

superior to that of other cultural fields, where the unification of the language and the publications is spread over many local journals with difficult access.

As it is well known, control of communications and practice of power are closely related. I do not think that I have discovered anything new with such an affirmation. Thus the system, far from allowing free publication of results among professionals, works hand in hand with censorship. Theoretically, it was conceived as a quality control but its functions are frequently extended to the control of power. Those researchers who want to publish in these journals are subject to the dictates of the chosen referee and the journal editors, who will say whether the paper is accepted or not. The referee, by choice, is usually anonymous. I had even a case in which the editor was anonymous as well, and we only knew the name of the secretary. This fact points out that the activity is not always honest. If it were honest, the referee would not hide himself behind anonymity or, perhaps, be afraid to be pointed out as the disparager. If somebody thinks they are giving good advice then nomination of their work for a journal should not be hidden.

Generally, papers are submitted to referees who are experts in the matter. They can afford their knowledge to improve the quality of the paper to publish, or detect errors in a calculation, if any, or detect contradictions with some data, etc. In principle, the idea is good, and it would be better if the refereeing process were always objective and impartial. I think that it is not the case. There are many cases in which the fate of a paper is dictated by a conflict of interests rather than the merits of the paper.

From my experience in the publication of scientific papers in refereed international journals, I have observed that the reports of the referees rarely detect errors in calculations or data reduction procedures, because the referees are not patient enough to carry out the calculations again or check the codes. Apart from minor details—changing a plot in order to see it better, explain a paragraph better, cite some other paper (in many cases the referee advises to cite some paper of their own, or by collaborators etc.)—, objections very often are to do with the referee's own opinion or how convinced they may be about the contents of what is going to be published. Generally, according to my experience and other experiences that I could list, the more controversial the topic, and the more challenging it is to established ideas and the newer the approach, then the more difficult the problems will be in publishing it, and the higher the probability of being rejected. Otherwise, when one writes a paper that repeats what has already been said by hundreds of papers on the same topic—with some changes, perhaps, in the parameters if a theoretical model, or focusing on different objects than those which have already been observed or observing the same objects with best data—and reaching the same conclusions which are already known and in agreement with everybody (especially the referee, who is usually representative of orthodox ideas; an exception might be in relation to some secondary points), in these cases, a referee will be more likely to be less belligerent and may even send congratulations to the authors.

The background problem is as follows: referees are persons who have dedicated their whole life to do research in the few problems of a particular field. They are widely recognized persons in their field and their social status is due to their contributions in the field. As persons with experience and prestige, and sometimes associated with an excess of vanity, they usually think along the lines of 'I am a great specialist in this field. I know the interesting and crucial ideas about it. If a new idea were presented, either it is of little interest, or it

is wrong, or I would have thought of it before. Therefore, if somebody presents a new work that tries to tap into crucial questions, either it is a continuation of my own work and ideas and those in which I was involved, or it is wrong'. Moreover, it might be misconstrued as competitive (argumentative, contrarian) for somebody to publish a theory or interpretations different to those argued by the referee. Perhaps it is somewhat exaggerated to attribute this thought to many "authorities of a field". However, I think that something like this thought is more or less present. Of course, this vanity is not explicitly recognized. The fact is that this psychological mechanism, although not explicit, can be present in most of the cases in which there is a discussion about the credibility or how convincing is a theory, or any other subjective approach. Certainly, science has an objective content, and the data and maths are there, independent of what is thought about them. However, the data interpretation and the plausibility of theories is something that is subject to the human factor—beliefs and, in many cases, prejudices. This carries much weight in the censorship of scientific publications. Of course, I must also say that there are many good referees who do a wonderful work as well.

What is the consequence of this? This really has a positive effect: avoiding the publication of hundreds or thousands of incorrect papers with absurd ideas that have no sense. Nevertheless, the negative side is also obvious: obstruction of thought and the few interesting ideas that could be produced. Well-done works but without ideas, works of specialist artisans, are rewarded. Creativity is damned. It seems that the system gives the message that no ideas are needed. It seems the system, with the set of higher authorities, is saying that astrophysics or any other science only needs to work out some details. It is accepted that the basis of what is known is correct, the present-day theories are more or less correct and only manpower is needed to fit some parameters or aspects of minor importance. A Copernican revolution is totally unthinkable within the actual system, even if truth were different to present-day theories. With regard to this, there are not many differences between the present-day academy and the university in the sixteenth and seventeenth centuries that conformed to the Church and Aristotle's texts. It is not true that science has similarities to an ancient religion, as has been charged on many occasions. Scientific arguments are very different to religious arguments. Nevertheless, the behaviour of human groups that claim to have the truth among their hands is very similar.

An important exception to this censorship is the existence of the electronic preprints "astro-ph" (or other names for other fields in physics or maths). This is, in my opinion, the most important effort to open doors to the research in which the originating author can place a paper without the control of a referee². Normally, papers are put in astro-ph once accepted in a major refereed journal, but the author can also put the paper in astro-ph before it is accepted by a journal. There are also some printed journals without censorship, but these

² Update: Regrettably, in the beginning of 2004, two months after I posted this paper in arXiv.org in the section of astro-ph, a new policy was established which curtails the freedom of dissemination of ideas. The new system requires that, if somebody who has never posted a paper in a section of the archives arXiv.org wants to do it, he/she must get the permission from a scientist who uses it regularly. Even if somebody uses often a section like astro-ph or quant-ph, he/she is not free to post papers in other sections unless he/she gets a permission of a peer after the revision of the paper to be posted. In the last years, the censorship system is becoming more refined: it is removing as possible peer reviewers the names of the people who give consent to post papers which are not welcome by the establishment. In my case, after giving support to some person who wanted to publish some challenging ideas (I thought these ideas were wrong, but they were worth to be published), I was informed that I cannot be a peer reviewer anymore.

are minor journals that are practically unread. Only astro-ph has a large diffusion. The counterpart of this freedom of publication in astro-ph preprints is that the papers are not officially recognized until they are accepted by a major journal. They cannot be used to support any proposal of time application in telescopes, for instance. They cannot be considered as papers to argue an idea against other approaches. They can even be ignored as if they did not exist. If a leading specialist is asked about a paper in astro-ph that is not accepted in a major journal, the specialist can simply reply that the paper was not published and, therefore, can ignore its contents as if it did not exist (this happens with many accepted papers too). Nevertheless, in my opinion, the astro-ph preprints are a good tool in the research. At least, another researcher without prejudices can read the paper and judge its quality. It is also useful for the author: in my case, I have found important errors in some papers thanks to the comments of a person who has read it in astro-ph; errors which could not be found by any referee, who are used to read a paper superficially, and reject it with a simple rebuff without good arguments when he/she does not like it. Of course, there is a lot of rubbish in astro-ph but there is a lot of rubbish among accepted papers in leading journals too. Somebody could also steal ideas, but that also happens with accepted papers that went unnoticed at their time and years later an author of prestige rediscovers them, and takes on the ideas. How many authors of the old Soviet Union have discovered many interesting things, which the world could not know until a clever North American researcher, with plenty of dollars, rediscovered it and Queen Ann's dead!³

Another problem is the number of papers. In the astrophysics branch, 30 thousand papers per year are published. This is a very high number, the reading of which cannot be undertaken by even the most hardworking of readers. Within a restricted sub-field of astrophysics, such as comets, Seyfert galaxies or others, one can find 500 or 1000 papers per year in relation to the topic, which is still a huge amount even if only to have a quick look. This number has grown up and continues to grow in an uncontrolled way over time. Chandrasekhar, one of the old editors of "Astrophysical Journal", after leaving his duties as editor, realized the increasing overflow in the number of papers per year. He used to say, ironically, that the increasing velocity of the paper number is higher than the speed of light, but there is nothing to worry about for there is no violation of any physical law because these papers carry no information.

Since most of these papers do not contribute anything important to the field but dispensable details, the possibilities of the few important papers that undergo censorship that would otherwise have an impact on the community are significantly reduced. This means that, once the obstacle of direct censorship in the journals is removed, the researcher who hazards new ideas will have to fight with an indirect censorship: the superproduction of papers that hide what is uninteresting to the system. Propaganda is the key element for a

³ Update: I can give a recent example with the research I developed in Tenerife (Spain) within the TMGS team. Between 1994 and 2003, our group has been publishing some papers on the existence of a long bar in our Galaxy with some peculiar characteristics [see, for instance, Hammersley et al. (2000, Mon. Not. R. Astron. Soc. 317, L45) or López-Corredoira et al. (2001, Astron. Astrophys. 373, 139)]. In 2005, a group of U.S. astronomers associated with the mining of the "Spitzer" satellite data published a paper about the discovery of the same bar with the same characteristics: Benjamin et al. (2005, Astrophys. J. 630, L149). Moreover, they produced a Press release with the title "Galactic survey reveals a new look for the Milky Way" which was divulged in many mass-media outlets. A "new" look? No!... this was proposed years ago by us. Benjamin et al. do not cite our works when they talk about the bar. According to some information which has reached us, they cited us in a first version of their paper (so they knew us; it is not a question of lack of information) but they decided to remove this citation and talk about their discovery as it were something new in order to save space.

paper to become known. For this, the leading specialists again have the advantage because they control most of the threads which move the publicity machinery; they have the appropriate contacts, they write reviews (summaries of scientific discoveries within a field), they organize congresses and give talks as invited speakers. Moreover, the reproduction of standard ideas has itself much more acceptance because the interests of those who work with them are many while the diffusion of new ideas is interesting only to their creators.

Congresses

The phenomenon of congresses, symposia, workshops, schools, meetings or any opportunity of joining other professionals to communicate and exchange ideas, has become widespread. The phenomenon is not only present in sciences but in any professional environment and has increased hugely during recent years. In the first decades of the twentieth century, while every month discoveries of huge importance were being made for the development of physics (for instance, in relativity, or quantum physics), such gatherings were celebrated once in a blue moon, with important international congresses held annually, or at longer intervals. Nowadays, in astrophysics alone—one of the multiple branches among all researches in the physical sciences—around 200 or 300 international congresses are celebrated per year, apart from small local or national meetings. The saddest aspect of the question is that the conceptual level of development of physics today is far below what was reached in the beginning of the twentieth century.

Holidays can be a reason to attend congresses. Many of them are celebrated in exotic or tourist destinations, which allow leading scientists and their friends to enjoy a holiday with public funds. However, the main purpose of congresses is not to promote tourism but the diffusion of the information in a micro-field of astrophysics (or any other science), and trying to give a wider, more global overview to a given topic. In order to do that, the congress is usually structured into long series of talks that last several days. The invited speakers are highlighted and they are allowed to give long talks, of up to one hour, to talk about their own research or those papers in which they are interested. They comprise ten or twenty leading specialists who are friends of the congress organizers or share kindred ideas. There are also selected speakers who are among those who apply to give a talk. Since their number is high (around fifty in a congress of three or four days without parallel sessions), the spare time is distributed, so that each of them can speak fifteen or twenty minutes. In this short time, they must discuss their research activities during the last two or three years. Consequently, the result is “concentrated” talk sessions that quickly exhausts the attention of the audience. Basically, they have the utility of propaganda. It is useful to say that I have carried out a work about this and the one who wants to know something of it must read my paper. Finally, there is a room for posters, in which hundreds of condensed papyrus concentrate texts and figures in a square meter of bristol board per poster to show results and obtain propaganda value from them. This reminds me of the trade exhibitions in which each company shows their merchandise for publicity. Moreover, publicity resources to attract the attention of the assistants at the congress (the same persons who show posters or offer talks) with pictures and poster designs of showy colors, videos of numerous simulations or films of some impact in talks (there are even cases of researchers who pay to professional animation creators such as “DreamWorks” to make the videos),

etc. All this has the goal to attract the attention of the audience who get lost among the tons of information; dispensable information since there is not much new to tell at each congress, simple technical details without too much relevance. The battle of the scientist is not finding new good ideas, but finding the way to sell mean, unworthy ideas. Marketing is more important than Math. It is all just publicity, and meeting colleagues to talk about, and discuss future collaborations.

This publicity is very important for the system and for the control of information flux. And it is important to give priorities in the congresses, provided that the first purge was already carried out in the research institute of the scientist. The scientist must first convince their own institution that the expense of assistance at the congress is justified by the contents that will be shown in order to secure a subvention. Sometimes, the assistance of certain personages is forbidden. For instance, in the conferences about cosmology at the Vatican in 1982, only the staunchest defenders of the Big Bang theory were allowed to participate, and marked individuals were left aside long with the defenders of opposing views such as Hoyle, Ambartsumian or Geoffrey Burbidge. As told by the physicist W. Kundt, “[cases] where I was not invited to topical meetings, and even where I was sent home from a meeting on the day of my arrival”. Fortunately, neither the Vatican example nor the experiences of Kundt are too extensive. There is a filter, but, even so, the censorship system is less efficient than the refereeing of journals and audacious theories pass censorship more frequently than is the case with the journals. Of course, when the proceedings book (which contains the written texts of talks and posters) is published, some authors can use only two pages in small format, or none, while others take up thirty or forty pages.

Financing, astropoliticians and supervedettes

Among senior researchers, not all of them have the same weight or authority in the hierarchy. There exist, as in any University department, certain ranks such as position in a chair, department director, etc. Apart from these nominal ranks, there also exist certain power status indicators that are associated with other factors.

Many senior scientists and functionaries with security of tenure devote most of their time to teaching at universities. Perhaps they take a ‘slave’—I mean a PhD student—to carry out work which the senior scientist will then co-jointly sign, in order to show that he actually does do some research. In first rank institutes, competition is higher. In those institutes, some of the researchers are leaders of a project, and pursue ‘impact’ a particular one, that is, the project have the objective of producing many published papers and they command a certain respect from the specialists.

The project’s main researcher is the leader of a group with several PhD students, several postdocs and, perhaps, some senior scientist of lower status. There are even cases in which this main researcher may have all the postdocs of a small institute. This main researcher is usually a type of commercial manager, where certain elements could be termed agent and adviser. I call them “astropoliticians” and have known several of them.

I think that most of astropoliticians exhibit a similar behavioural pattern. One must make an appointment to simply talk with them, since they are always busy with a thousand and one tasks. “I have no time” is one of the favourite sentences of the astropolitician, a man of our era. We live in a time in which even the pipsqueaks pretend to conduct themselves and take over as if they were important men (a minister or someone who is very important) and deliver the self-important response of “I have no time” or “I am busy”.

Within their offices, it is usual for them to receive three or four phone calls in less than thirty minutes. They receive tens or hundreds of e-mails daily. When an appointment is required in order to present some scientific results for an opinion, the astropolitician has to revise their agenda, mentally or in a notebook, because there is always a meeting to be attended somewhere. In addition, much travel is undertaken both nationally and to foreign countries. They must prepare talks, because they are the main speakers at the various congresses. They must attend a large number of meetings of astropoliticians to obtain agreements (scientific collaborations, not commercial agreements, but the outcomes are similar), or negotiate some budgetary entry, or create propaganda for the project in order to obtain some economic benefit or achieve an impact in some other way, or to think up—together with other astropoliticians—yet another macro-project that will cost many millions of euros and will employ the many researchers in yet more monotonous work. Of course, they are not these researchers but the persons who are subjected to their orders—along with other new slaves who will be brought on stream with the money received from various negotiations. When astropoliticians are not travelling or in a meeting, they usually are busy with the preparation of periodic information bulletins concerning project activities or filling in forms to apply for new telescope time (which may also be done by students) or applying pressure for economic support for the project (travel, computers, scientific instruments, etc.) on some ministry or other for various types of assistance. In their spare time, they usually are busy with the coordination of project staff and their work efforts as well as establishing research priorities. The hen takes a cup of coffee to rest from the bureaucracy duties, while the little chickens are all around, eager to show their results. The astropoliticians listen to (in many cases, they do not listen to), and read papers by low-ranking ‘workers’ and express their opinion and, more than likely, suggest changes according to their prejudices. It is a rare day when the astropolitician may sit down to do some actual scientific work in the true sense of the word “scientific”. Perhaps, they may dedicate a few hours on some days to teach an aspect or some feature to a low-ranking science worker. In most cases, however, it is not they who dedicate months to work on resolving various problems but, instead, their PhD students or postdocs.

When an astropolitician is able to display particularly bright attributes as agent and trader for the science that created these workers, we have an example of a “star”. A star that is dazzling in both its brilliance and prominence. In other words, a “supervedette”, the great star among the stars. In an institute with more than a hundred researchers, there are usually only one or two supervedettes. Their identification is not very difficult because they are an essential reference of the particular institute, especially in the image given to the world outside. If a journalist visits in order to write an article about what is happening at the institute, the supervedette comes to the fore. If his/her team does any work, the press is quickly called in to announce to the world what So-and-so “et al.” (that is, “and collaborators”, although the name of the low-rank workers

who has made the discovery is usually not important), with the phenomenon of fame being confined to the supervedette. They have usually a good eye for choosing the topic that has high popular impact (not necessarily topics of high scientific importance). If the topic is without fuss, they will announce it with a lot of ballyhoo in order to create a fuss. They publish without difficulty in the journals; they write professional and popular books. They are the owners of the congresses together with others of similar ilk, they get all the telescope time they want, and the budget for their activities is gargantuan. These persons do not think in terms of minor but major goals. They lead large multimillion-euro projects. In addition, a single phone call can translate into widely disseminated propaganda via journals and television. They are in national and international price competition as the ‘best researcher’. They are mixed with famous high society personages. Circumstances may vary depending on how big is the “star” but he/she is—in short—a basis of envy for any astropolitian.

Of course, there are exceptions to this behaviour. Any attempt at generalizing a behaviour pattern of a given collective is always subject to the corresponding corrections for the particular details of each case. There are some cases in which a senior scientist, even a leader of a project, works at the same tasks as do lower-rank workers, and does not dedicate too much time to administrative tasks. This, however, is not the most customary scenario.

I have the particular case in mind of a senior scientist with secure tenure that is a good example of such exceptions. This person does not lead any project. He works with his own ideas, or together with a collaborator. This person goes to very few congresses (perhaps one every five years or less). He does not go to meetings. While talking with him in his office, he has few phone calls. He spends a short time in answering e-mails. He always has time to receive visitors to his office when somebody wants to talk with him. Apart from working as a good professional in his field, he has a wide knowledge of many subjects and many other fields of study. This makes it possible for people to speak with him about a physics topic or on any one of a number of other fields, since he knows a great deal about philosophy and history and has a very good memory of what he reads. He is accustomed to thinking. In fact, on many occasions when visiting him in his office, I found him actually thinking; not performing some task related to administration or self-promotion and talking on the phone, but thinking. When we speak, I usually discover, in the lucidity of his thoughts and reflections, the answers to many aspects of astrophysics. He thinks quickly (perhaps too quickly for a listener to understand what he is thinking as his thoughts are being articulated) and is almost always correct. He has great intuition and visualizes a problem in order to discern its elements. His help is always likely to be given when one has difficulty in solving a problem in physics. This person is a prototype of the learned scientist and is quite rare these days in our professional scientific jungle. He certainly is not mainstream and, in fact, is considered a second rank scientist. Few people know of him outside the institute, and his complex works are nearly always forgotten because of the absence of any accompanying propaganda. Nowadays (and most likely in the past as well), the ‘trumpeter’ is another species of scientist: the executive with attaché case in hand; the professional science agent. In many cases, this agent does not know how to think about solving a scientific problem, nor is there too much insight or knowledge about physics and astronomy. Many findings by members of astropolitics do not deserve to be highlighted from an

objective standpoint, but this objectivity is difficult to achieve with all researchers believing their own works are important. Therefore, the astropolitician is the triumpheer.

Press, television, propaganda

As mentioned earlier, press, radio, television or similar media, are useful tools for the manipulation of information and mercantilist propaganda. The knowledge of society in relation to scientific activities in general stems almost totally from press, television and propaganda sources. Therefore, control of these media is an ideal mean of achieving what the controllers of a society desired to have perceived or believed by the general public.

Most journalists responsible for writing articles about science have little knowledge about what they write; perhaps they have some knowledge on science in general, but they are very far from being in control of all the existing specialties. This is the situation in even the most prestigious newspapers in the country. For the less prestigious ones, it is even more likely their journalists have no scientific culture whatsoever. Because of this, the journalist is obliged to believe what the researcher says. If they are told that a great impact discovery has just been made, the journalist must trust that it is so, since the journalist has no personal knowledge from which to cast doubt on the veracity of the researcher's statements. The determining factor in these situations is the researcher's reputation. Thus, fame feeds fame: a prestigious researcher is usually surrounded by a swarm of journalists. The propaganda they distribute will contribute to increase the 'fame' of the researcher. In this regard, there are not many differences between the 'fame' achieved by a scientist and that attracted by a singer or a protagonist of the pink press: it is all question of availability to the mass media.

Researchers perhaps overestimate the value of their own work, but do not usually deform or exaggerate, or say the opposite of what it is—at least not intentionally. The journalist does these things and does them intentionally. The goal is the impact, which is something of high value among friends of misinformation and ballyhoo. It is, apparently, what they are taught in the faculties of journalism. Thus a good deal of the information published in the press about recent scientific discoveries contains significant errors and receives appraisals which are totally inconsistent with the purported newsworthiness of the reported item. Titles--headlines often distort the news. I still remember reading in a newspaper something like "extraterrestrial mummy", in reference to the fact that some researchers had found the tomb of an ancient Egyptian pharaoh that had been built with stones from a place where an extraterrestrial meteorite deposited on Earth in the past. Incidentally, it seems that those who are ignorant of science are usually worried about extraterrestrial life and, therefore, journalists feel their duty is to satisfy the readers who are eager for news related to the subject. Perhaps because of this, there is much news published about the discovery of new extra-solar planets. This usually presents the news item as if it were the first time an extra-solar planet had been discovered, instead of the true facts that are that tens of them have already been discovered and all with masses thousands of times greater than the mass of our planet. These 'masses' have nothing to do with earth-like planets. And, no! extraterrestrial life has not, as yet been discovered—the most common question that

journalists make, when talking with the populist mouth, in their eagerness to convert science into a side-show.

The number of cases where scientific news is published with a disproportionately huge amount of ballyhoo—such as Einstein’s theory of relativity is no longer correct, or that there is life on Mars, or cold fusion, or similar is very high. In many cases it stems from the interpretations made by journalists because they do not understand the subject. In other cases, the sources may be real discoveries that have been published in scientific journals, but which are still being discussed and about which certain controversies remain. After some months of the sensationalistic publication, the scientific community usually clarifies that the discovery was not such a discovery because there were some errors in their results. However, general public only remember the huge ballyhoo, not the reply that refutes it. Apparently, the truth is not so interesting for commercial purposes, and does not help to sell further newspapers or magazines. If somebody wants to know something about science, I would advise not to do it through the press or television, but through textbooks. I would also advise them to forget the newspapers. Future will tell us what is being done right now.

In spite of the imperfections of scientific communication throughout the mass media, it remains the fundamental pillar of the relationship between scientists and society. Many of the subventions of multimillion-euro bequests depend on it. For example, the case of the Antarctic stone with a life of Martian origin was famous all around the world. It gave rise to a large subvention for further research into the topic from the American government. Afterwards, the news was denied—the stone was contaminated with terrestrial life—but those who got the money for the project had already obtained what they wanted. Incidentally, the paper about this discovery was published by ‘Nature’, a professional journal of prestige. The paper itself had been submitted to three or four referees and accepted.

In some cases, the opposite thing happens. Subventions are not a consequence of the press, rather the press is a consequence of subventions. When large amounts of money are invested in a project (the sum may be as high as hundreds of millions or euros or even billions of euros), justifying the investment of public funds becomes necessary. Therefore, the press is usually called in to explain to the nation the great discoveries obtained, with thanks to the taxes paid by the nation’s taxes. This, again, is propaganda. In some cases there are somewhat more important discoveries, but in many cases there is nothing interesting. Specifically, in the last examples the press is needed to exaggerate a matter and create the belief that the items newsworthiness is more important than it actually was. Things are said, such as so and so many new galaxies in the Universe have been discovered, as if the old surveys had not encompassed millions of galaxies yet. When an ignorant general public, most of whose members do not even know what constitutes a galaxy, reads such news, they become convinced of the greatness of the stated venture undertaken by the survey.

Moreover, the press is not always at the service of all-important scientific phenomena. Without fame, without money and without the recommendation of, or support from, a prestigious team of researchers, even the best of scientists, working in the most important fields, would be not listened to, nor paid any attention. Thus, yet another factor arises to account for the isolation of the non-mainstream scientist.

“An individual with few resources getting what we could not get with billions of euros. This would be a scandal, and we cannot allow it”. This is the message of the actual capitalist society where money imposes its power.

Telescope time

Astrophysics, as with all sciences, has a theoretical part and an experimental/observational part. At present, it is restricted to the observation of nature. In this science one can see but one cannot touch. For obvious reasons, experiments cannot be conducted with astronomical objects. There may be great advances in research due to purely theoretical work. However, it all depends finally on the contrast between such theoretical work and observations. In order to be successful, a theory must be able to predict certain phenomena that other theories cannot explain. Even Einstein's general relativity had to await the observational confirmation of the starlight being bent by the gravitational field of the Sun in order to have the impact it had—it was measured in 1919, by Eddington et al. Indeed, Crommelin, one of the lower-ranking of co-workers, together with other collaborators from Brazil, carried out the higher precision measurements, while Eddington's group in the Spanish Guinea had severe weather and were unable to obtain such precision in their measurement data. Therefore, advances in astrophysics advances are closely related to observational advances.

At the beginning of the twentieth century, astrophysics, and science in general, had made important progress due mainly to the search for new ideas by several famous researchers. When Hubble and Eddington were asked what they expected to find with the new five-meter telescopes that were going to be built, their reply was “if we knew the answer, there would be no purpose in building it”. Nowadays, however, the situation is very different. Before using large and even not so large telescopes, a tribunal of specialists must be convinced that something will be found which is already expected. In order to use these great installations, some forms must be filled up between six and twelve months before the observation date. In these forms, one must clarify what finding is anticipated as a result of the application of their measurements and observations. The tribunal of specialists must be shown the purpose of pursuing the observations. In addition, profile data on the researchers must be filled. Of course, the greater the history of observational publications and telescope time that a researcher has had in the past, the higher the probability of gaining further telescope time. Therefore, the researcher will be able to publish more papers than anybody else, although all the papers are similar and without any worthwhile, or new ideas – but prestige will be increased and enhanced. Using a large telescope or satellite to obtain data also adds prestige to the published results. One says, for instance, “data obtained with Hubble space telescope”, and this serves to presume that these data are far worthier for science than any other information gathered with less prestigious telescopes. It is a circular loop, and one just needs to establish a certain level of congeniality with the established leading scientists in order to enter that circle. From my own experience, and another whom I know, in the sending of proposals the probability of gaining telescope time increases very significantly when one of the co-authors of

the proposal is among the members of the tribunal (although that member will not actually be judging the particular proposal).

Nonetheless, the biggest problem for the advance of science is that new ideas are not welcome among the tribunals that authorize telescope time. If somebody applies for telescope time in order to test the predictions of an alternative theory, rather than the standard one, the proposal is most likely to be denied. We are not talking about amateurs for whom some crazy idea has occasionally gone to their heads; we are talking about great professionals whose only defect is in doubting the ideas which all the rest of scientific thinking considers untouchable. The system does not support an ideological plurality within the science. It is said that there is freedom in research, but this is just a lie as are so many other statements made by politicians, ostensibly in the name of the democracy. Of course, anyone can think what he wants to think, but the installations, the prestigious publications and the propaganda are only for those who want to make a science a reconfirmation and underscoring of certain prejudices, rather than an opening towards new horizons.

It might also happen that somebody presents an idea or an objective that is interesting for its study, but the work cannot be developed because the tribunal does not make telescope time available. Immediately, people from the tribunal with further resources, seeing something interesting in the idea, begin to develop it and make the discovery their own.

All this is understandable, although not acceptable—at least from my standpoint. The scientific body politics is convenient for large flows of capital. A telescope such as the 10-meter one that is being built in La Palma with a Spanish budget costs the huge amount of around one hundred million euros. If one looks for high amounts of money, space telescopes and the great satellite research projects costs go beyond one billion euros, and are paid for by several countries. Apart from the building costs of the telescopes and satellites, there is also the maintenance expense. In total, taking into account the average life of a large ground or space telescope, each observation hour costs thousands or tens of thousands euros. Therefore, it stands to reason that the use of the telescopes by the first barmy person who promises the moon and stars should be avoided at all costs. Moreover, nowadays, in order to obtain such huge amounts of money, it is better to show a solid image of science, an image that indicates science knows where it is going and has a clear view on problems that have been solved and those that are to be solved when money is available for research and instruments, etc. The image of a pluralistic science, entrenched in discussions about fundamentals, is not sufficiently interesting to attract investment money. Huge amounts of money are not invested for the purpose of allowing the scientist to play at guessing how nature is. Those sums, however, are invested in order to obtain a firm product far away from the speculative wordiness of philosophers. In other words, science is bought, and the one who pays has the right to demand the fruits are superior to those obtained for a lower price. All is accounted for with regard to the sums: the number of publications obtained with a telescope, the number of citations obtained by those papers—called “impact”, which is a parameter as related to the quality of a work as is the number of people comprising an audience for various television programs. In the end, informs must talk about how profitable an investment has been. Parameters such as genius, creativity, mental lucidity and other human factors are not included in these informs.

Spontaneity has no place in actual science. Neither, is there a place for fortuitous discoveries favouring those who suspect that something in astrophysics is not following an appropriate path. Almost everything is planned in order according to programming and forecasts made many years in advance (it takes around fifteen years from the beginning of the plans to build a satellite until it is launched). Predictability was seldom present in the long history of science. Many times, science has had to walk back a way, to retrace its steps, before taking some new way or approach. Many surprising discoveries have been done by pure chance. However, contemporary system is apparently surer than the science of any previous time in that there are no historical errors; at least, if there are some errors, then the system tries to delay their discovery as long as possible. It is an apparent paradox that the greater the possibilities of science to observe and make experiments, the greater the obstacles—rather than motivation—for its advancement. Does astronomy move backwards with the advance of technology? There is no doubt that astronomy is an observational science that has larger “possibilities” with better instruments. However, the control of science by the system is larger when larger telescopes are available, so private initiatives are blocked if they are in disagreement with established standpoints. In this sense, astronomy regresses. That is, the ‘possibilities’ escalate but the efficient use of these possibilities declines. Telescopes do not ‘think’ alone and produce casualties when advances in technology result—as is often the case in some sectors of science—in mental atrophy.

Advance/stagnation of science

There are many real examples that can be given; many names, and many problems related to astrophysics that were manipulated in favour of a given trend and where the prejudice against alternative approaches can be illustrated. The discussion of these particular theories is not the issue in this paper. Nevertheless, I can speak about some instances of general trends. For instance, the almost exclusive use of gravitation as a basis of understanding many astrophysical problems dealing with the large scale cosmology, galactic dynamics, and formation of large-scale structure, etc. There is a quite strong stream towards this direction. There are alternatives, of course, and we could take, for instance, the case of electromagnetic interactions at large scales, but working with these forces needs a much larger effort than simply working gravitational forces. The uncertainties about magnetic intergalactic fields, for example, are huge. Then, what is to be done? A devil-may-care attitude is predominant. Gravity is used to try to solve these problems and, when magnetic fields are mentioned, pained faces show up as well as expressions indicating “one should not make life difficult for us; we are happy with what we do”. However, nature is difficult to understand, and truth may have nothing to do with the positions taken by some scientists who do not want to have disturbed the peaceful tranquillity of their lives. Ockham’s razor is usually cited by many who are often pretending to emulate a philosopher. Apparently, Ockham and his razor is the only philosophical reference that many scientists can lay claim to but it is often cited inappropriately because nature’s simplicity is confused with the simplicity of what they can calculate. “Nature does not care for analytical difficulties”—said Fresnel in 1826. There are many problems in astrophysics and gravitational interactions are not always the answer. In order to resolve the situation, leaders in scientific research push science toward speculative ways with terms such as

super-massive black hole, non-baryonic dark matter, inflation, cosmological constant, gravitational lens which are some times not even understood. Nonetheless, they continue to speak in gravitational terms of reference rather than opening up to, and learning new branches of physics from those to which they have dedicated the past twenty or thirty years. In considering large issues such as the luminosity of quasars, they claim the existence of invisible large black holes, with millions or tens of millions times solar masses. They forget Ockham's razor and it does not matter that they have to use all the patches at their disposal to fill the gaps created by their prejudices. All is possible, except the taking of leave, a departure, from their prejudices. The same thing happens with the topic of intergalactic extinction of which exact knowledge has not been achieved and which, for convenience, is taken as null in all wavelengths up to very large distances. Another example is quasar distances, commonly accepted to be the distance derived from the redshift which is interpreted as being cosmological, in spite of the problems which this interpretation has in explaining certain observed correlations between nearby galaxies and distant quasars. There are other problems that are avoided by looking away, elsewhere, as soon as they are mentioned. Is there insufficient visible matter to justify the kinematics? No problem; dark matter is introduced and everybody is happy. Research advances until it is realized that dark matter cannot be any known matter. Then, another patch is introduced in the established theory and new types of never-before-seen matter are invented: non-baryonic dark matter, which is also useful in solving problems in observing CMBR anisotropies a thousand times lower than expected before the taking of measurements. And inflation is invented, and the cosmological constant is whimsically put or removed in Einstein's equations, according to the fashion, and more and more free parameters are added to a theory in such a way that, if something does not fit the observations, it is a question of changing parameters ad hoc. And when will this fashion of patching the theory, to ensure it accommodates those results (that needed to be ironed out) in order to make a posteriori predictions, be finished? Perhaps, when somebody realizes that there is a failure in the base premise of the piece-by-piece-built construction. Then, it will be the time to throw everything into the dustbin and begin again in some other place. A very clear historical example comes to mind in the Copernican revolution that tore down the highly patched astronomy of Aristotle-Ptolomeus. This is precisely what is presently being pursued simply for avoidance, at all costs.

Maybe all the "alternative" theories are wrong—maybe. Nevertheless, can we be one hundred percent certain that the standard scenarios are correct in order before we reject systematically all the alternative proposals just because they are against an orthodox view? I do not think so. However, the system acts apparently as if it holds the final theory in its hands. The system has a set of modern patched theories, like those of Aristotle-Ptolomeus, and it is afraid of the loss of its privileged status. Galileo had to fight hard against the mainstream in his time, and the passage of history has, in many respects, changed little. In fact, it seems that nothing changes. It is pitiful that nowadays propaganda sells us the idea of freedom that is so far away from the circumstances of four or five centuries ago, yet we really live with the same dogs, although in different collars. At least we have progressed somewhat for certainly nobody is burnt at the stake. At worst, somebody may be exiled from their kingdom and life made impossible for someone in order that they do not publish or otherwise advance in their research. It is also pitiful that an image about cosmology, for example, may be sold such that everything is perfectly clear and only a few parameters remain for high precision

fitting. It is probable that the basis on which actual cosmology (a relatively young science, if it can be called a science at all) has been developed is completely incorrect. However, the system continues to build, rebuilding itself ever more quickly over ever increasing quicksand. It seems that nothing has been learnt from history; that the economic interests which power the business of science are conveyed into thinking that a solid knowledge is firmly treading the 'good way'; into taking that 'way' forward regardless of the possible sabotage and disagreeable, critical elements.

When all these arguments are related to an orthodox scientist, the answer is usually that science is objective and, therefore, a first theory is supported rather than another second theory, because further proof was obtained in favour of the first one, rather than the second one. These words sound very nice, and they even appear honest. However, in the light of all that has been said in the present paper, one must consider that not everything is so honest nor do I say that everything is pure manipulation either. No, there are many cases in which nature shows itself clearly enough in the experiments and observations, and the conclusions are irrefutable. But there are many turbid cases, belonging to turbid sciences such as cosmology, in which the power of manipulation is stronger than nature.

It is true that certain standard theories work better at explaining the data. However, the number of persons involved in a particular theory, and in patching it here and there is not generally told. It is said that the Big Bang theory, for instance, has defeated competing theories. Of course, this theory has thousands or tens of thousands of researchers who in some way are involved or interested in the theory being correct otherwise their work of their lives would be jeopardized. Whereas, an opposing theory may be defended by a small number of researchers who can be counted with the fingers of one hand. Even if they are very good scientists, they cannot compete to produce patches and to spread propaganda on the same scale as do the huge numbers of orthodox researchers. These researchers have to fight against the system without money, without students, without telescope time. Personally, I think that cosmology is not a serious science and I do not believe any theory, neither the Big Bang nor the competing theories. In any case, to place these conditions in the context of a sporting framework, the game is not fair nor does it seem appropriate to talk about defeat when the real issue is abuse.

Objectivity in the scientific method is usually aimed at a target. However, in turbid matters, the method to be applied is usually not very objective and basically is as follows:

- Given a theory A self-called orthodox or standard, and a non-orthodox or non-standard theory B. If the observations achieve what was predicted by the theory A and not by the theory B, this implies a large success to the theory A, something which must be divulged immediately to the all-important mass media. This means that there are no doubts that theory A is the right one. Theory B is wrong; one must forget this theory and, therefore, any further research directed to it must be blocked (putting obstacles in the way of publication, and giving no time for telescopes, etc.).
- If the observations achieve what was predicted by theory B rather than by theory A, this means nothing. Science is very complex and before taking a position we must think further about the matter and make further tests. It is probable that the observer of such had a failure at some point; further observations are needed (and it will be difficult to make further observations because we are not going to allow the use of

telescopes to re-test such a stupid theory as theory B). Who knows! Perhaps the observed thing is due to effect “So-and-so”, of course; perhaps they have not corrected the data from this effect, about which we know nothing. Everything is so complex. We must be sure before we can say something about which theory is correct. Furthermore, by adding some new aspects in the theory A surely it can also predict the observations, and, since we have an army of theoreticians ready to put in patches and discover new effects, in less than three months we will have a new theory A (albeit with some changes) which will agree the data. In any case, while in troubled waters, and as long as we do not clarify the question, theory A remains. Perhaps, as was said by Halton Arp, the informal saying “to make extraordinary changes one requires extraordinary evidence” really means “to make personally disadvantageous changes no evidence is extraordinary enough”.

Unofficial science

The system really invites being left alone. I am actually convinced that if somebody wants to make something important—here, again, I remark that this is not only applicable to the sciences but, in general, to any human Mafia with the name of ‘culture’—it must be done away from officialdom, and perhaps in free time and laborious study by oneself. The problem for the sciences with this position is thence the precarious or even nullified possibilities available to thus observe or make experiments, not to mention the bad reputation associated with free-thinking occurring away from the official institutions. Since the expenses for the necessary materials are very high, the possibility of doing high-level empirical research from the periphery, in any field, is practically nought. The only possibility is pure theory/speculation, or perhaps feeding of empirical data produced by other scientists which is, in fact, quite frequent.

I receive very frequently—nearly every one or two months—by e-mail new theories from amateurs who try to throw down all the well-established physics to leave space for new and often ridiculous theories, or cases of cosmological theories that are failures in even the most basic aspects. This kind of work has practically no reference in professional journals, and tends to cite popular books on science. Rather than studying a particular scientific problem, they talk about very general matters. For instance, they try to throw down all the known physics. Precisely because of that, the independent research carried out away from official institutions finds problems of credulity; for each researcher with enough preparation who wants to do serious things, there are thousands of ‘barmies’ on the planet who dream of creating a theory of physics inspired by the heavens such as poetry, which demolishes all the past and opens a new era in the history of science. I once heard on a radio program an interview with a carpenter who had never studied physics, but had just read some popular books on physics, without trying to understand anything about maths. The carpenter said that he had written seven books about black holes, and he complained that he could not publish any one of them. I do not want to judge negatively the efforts of some amateurs, who perhaps have read some popular-science book by Hawking and think that they are able to work as researchers. I do not want to act as a part of the system that castrates any attempt at originality just because it is challenging. Nevertheless, the reality is that amateur’s theories have a lot of failures and inconsistencies because they have no knowledge independent of the ideology. And the result is that the thousands of ‘barmies’ in the

penumbra do not get to listen to the voice of some possible genius who could be in their midst. Therefore, autonomous research activities do not have a high credibility, and one must use official mechanisms in order to be listened to by other professional researchers.

Attitude of philosophers to science

It is probable that some will identify the present manifesto as a philosophical criticism, a charge made by many against science. I think that the present way of thinking is philosophical. However, it must not be confused with the types of presentations made by the self-claimed professional philosophers i.e., those who have an academic degree.

Indeed, it is not often one finds this type of criticism about science. There is criticism, of course, but very detached from contact with the problem and often no more than a paraphrasing of metaphysical speculation that has very little to do with the above mentioned problems. The philosophy of science as nowadays taught in the faculties of philosophy is, indeed, a philosophy of anti-science. It is taught that scientists are inept and do not know how to think while the professional philosophers are those who are able to give sense and meaning to science. There are several approaches to the philosophy of science that I will not be discussing because that is not the goal of this paper. There is a wide range of positions that could be taken. For example, the openly anti-scientific position that compares science with religion and holds the view that there is not any truth in scientific knowledge or that the science of an African tribe's witch doctor is comparable to western science. Then there are the less crazy ones, limited to explaining to the world—in very thick volumes—what a hypothesis is, or the falsability of a theory, and those trivialities which are well known to any scientist since early education, and which do not reveal anything not already known. Apart from these efforts, professional philosophers make very few attempts to understand the present-day problems of science and perhaps, some of these problems are only mentioned in order to discredit science in general. Sentence such as “this agrees what we had said...” and trying to sell some of the metaphysical and paranormal (in Spanish “para anormal” means “for an abnormal person”) creeds that are usual merchandise of modern sophists. A Spanish proverb says: “under the heavens, everybody lives on one's work”.

Why is this kind of criticism so infrequent among the works of professional philosophers? I think there are two main reasons: 1) they do not have knowledge of the science from close quarters but through reading books which do not reflect the real problems; 2) they are not interested in revealing the problems of another profession because they themselves share the same problems in even greater magnitude. Obstruction of the freedom to initiate a research line or ideology is more prevalent in the faculty of philosophy than in science. Philosophy congresses are simply imitations of scientific congresses. Censorship of publications is more evident (most being confined to local town dissemination rather than international); they have practically no objective criteria and there are no empirical data, so a paper can be rejected whimsically, without even producing information as to the cause or reason for the rejection. Work positions are nearly always handpicked. Communication with the press, or promotion for the publication of books by editorialising is in the service of the corresponding supervettes. Propaganda decides the survival of philosophical nonsense,

etc. In this panorama, what has the ‘office-philosopher’ to say about science? Therefore, it is not strange that there should be silence about these aspects. They prefer to dedicate their efforts to ascertaining what is the meaning of ‘truth’ or how many types of reason exist or many exercises of language analysis or the classification of the different schools with different “-isms”. As has been said, under the heavens, everybody lives on one’s work. The problems of science are not going to go away, nor are they to be resolved by any paid philosopher. These questions are things to be discussed by scientists themselves, and from the inside looking out.

Some final optimistic notes

In short, I see with certain pessimism the actual state of astrophysics, as well as of other sciences with similar problems. All the circumstances above described may lead one to conclude, that the actual ‘product’ from the branch of science known as astrophysics has become prostituted in many senses. According to the dictionary, one of the meanings of ‘prostitution’ is the use of talent or ability in a base and unworthy way, usually for money. This is what astrophysics and world’s oldest profession have in common. These problems are reflected in many other fields of culture, as well as in our own society. Everything produced contaminates everything else. There are no isolated problems; any human activity is a reflection of the environment that surrounds it. We live in a rotten society that deceives itself. What else could be expected from science in such a society?

I recognize that my criticism is not objective. In contrast with the exaggerated optimism of other, perhaps my pessimism is exaggerated too. Perhaps my view is somewhat disproportionate. Well, each can be judged. I am simply expressing my opinion and everybody has an opinion. This is not a pamphlet with political or sectarian ambitions. I am not interested in convincing anyone of any claim. I do not think that this text is useful for trade-union claims that demand the rights of science workers. Rights are not the issue here but facts are: to know how nature is. Making high quality science is the issue here. It is not helpful to claim some “right”, because the present problems of scientific research will not be solved with the increase of bureaucracy; they would simply worsen. Neither is it a matter of asking for further money to solve the existing problems. Quite the opposite: the more money is invested, the more the system becomes a Mafia. I am pessimistic about even finding politically correct solutions in the actual social context.

Anyway, I do not want to finish this text without arguing that everything is not black and although not totally satisfying, there are certainly reasons to have some degree of optimism. Truly, in spite of the problems that exist in the scientific infrastructure, I think that science can be made and there is a net advance. It is not the huge advance claimed by propaganda, but there is some advance. It is slow, with many errors that are very slowly being corrected, but there is some advance and our knowledge about the universe is maturing. In other epochs, there were also many difficulties that were barely overcome. However, it seems that there is a historical mechanism that with the independence of human interests, polishes and filters the most solid of knowledge as time goes by. Probably, it is because the created interests vanish gradually with the advance of generations, and it is only after some tens or perhaps hundreds of years, that ideas with

intrinsic value are distilled and survive to present us with their wisdom. Indeed, history is not always fair. Many good ideas are forgotten and are not recovered until they again rise to the surface of independent thought. Copernicus had to rediscover what Aristarchus of Samos knew seventeen centuries before. There are many cases of historically famous researchers who have stolen merit from persons unknown. Neither is history itself perfect for, after all, it is also human. Nevertheless, I believe that there is something great in astronomy, in physics, in all the natural sciences that allows the human being to look beyond its present place and to arrive at some understanding of what goes on beyond the insignificant meanness of spirit that so often pervades our existence. There is a Nature; there is a Cosmos; and we walk towards the understanding of it all. Is it not wonderful? There are many charms in the profession; as many charms as in love provided, of course, that they are not in the service of mercantile aims.

The Last Scientific Revolution

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Abstract: Critically growing problems of fundamental science organisation and content are analysed with examples from physics and emerging interdisciplinary fields. Their origin is specified and new science structure (organisation and content) is proposed as a unified solution.

The end of a lie, or what's wrong with science

Whereas today's spectacular technology progress seems to confirm the utility of underlying scientific activities, the modern state of fundamental science itself shows catastrophically accumulating degradation signs, including both knowledge content and practice [1-58]. That striking contradiction implies that we are close to a deeply rooted *change* in the whole system of human knowledge directly involving its *fundamental nature* and application *quality* rather than only superficial, practically based influences of empirical technology, social tendencies, etc. Science problems, in their modern form, have started appearing in the 20th century, together with accelerated science development itself [59-64], but their current culmination and now already long-lasting, well-defined crisis clearly designate the advent of the biggest ever *scientific revolution* involving not only serious changes in special knowledge content but also its *qualitatively new character*, meaning and role [41-47,53-58,65]. We specify below that situation, including today's science problems, related development issues and objectively substantiated propositions for sustainable progress.

Summarising *critically growing problems of modern science*, it would be not out of place to begin with the *internal science estimate* by its practitioners and dynamics of its “human dimensions”. Increasingly dominating *mediocrity* of results, human choices and relations in today's *fundamental science* is expressed in a huge variety of public or private opinions (e.g. [1-47,51-55]) and presents a striking contrast not only to simultaneous triumph of empirically advancing technologies, but also to a previous, very recent (decades-old) and visibly huge, euphoric success of the same science, with ever brighter perspectives appealingly looming ahead. But more than ever “pride goes before destruction”, and the haughty spirit of seemingly omnipotent knowledge is “suddenly” transformed now into a dirty fight of vain ambitions accompanied by the “discovery” of omnipresent ethical decay in science practice (direct fraud or “officially permitted” lie,

organisational corruption) [21-46], rather than any novel truth about reality. We reveal below the exact, rigorously specified reason for, and *true* meaning of, such dramatic “end of science” (cf. [3]), as well as much more positive perspectives of knowledge development far beyond its now ending, *unitary* level [42].

We show that both scandalously stagnating old scientific problems, its supernatural “mysteries”, and increasingly accumulating, catastrophically big new paradoxes result from the *artificially limited* scheme of officially dominating, “positivistic” science approach, where the unreduced, interaction-driven, *dynamically multivalued* reality is replaced by its *effectively zero-dimensional (point-like) projection* [42-44]. That huge, *maximum possible* simplification of reality within the *subjectively imposed* unitary doctrine explains both its visible (though always strongly *incomplete*) success at the *lowest* levels of *complex* (multivalued) world dynamics and even more evident (and this time *complete*) failure to provide objective world description at *higher complexity levels*, marking the border between “exact” and “natural” sciences (let alone “humanities” and arts), which is only formally postulated in the official knowledge framework. Thus *scientifically* specified origin of conventional science limitations exceeds essentially their existing empirical descriptions (e.g. [3-6,23-25,47,59,64]) that often correctly present various aspects of resulting knowledge degradation, but fail to trace its genuine meaning and related perspectives of positive change.

It is not surprising that a completely deformed, over-simplified picture of reality, not even attempting at its genuine, causally complete understanding, is imposed with the help of equally strongly simplified, *authoritarian organisation* and *criminal practice* of science today (see e.g. [7-14,26-41]). Indeed, what is the sense to ask for more ethics in research within today's bankrupt scholar science, if this *particular* kind of knowledge is *based*, starting from its most fundamental levels, on explicit and evident trickery of supernatural “mysteries”, *officially postulated* within an allegedly “objective” and “rigorous” doctrine? The effectively zero-dimensional, *zero-complexity* “model” of dynamically multivalued, *high-complexity* reality, totally dominating and *artificially imposed* in all research and teaching institutions, is none other than officially legalised, *maximum possible lie* about the real world structure and dynamics. All previous and modern *science wars* and *unethical* cases of science practice are but manifestations of that single, basic *departure from the truth*, the latter always remaining, however, the officially announced purpose of science. We show how the necessary transition to the new, unreduced form of knowledge – exemplified by the *already created* framework of the *universal science of complexity* [42-45,66-76] – naturally involves the related deep change of science organisation and practice towards an *explicitly creative* system of knowledge production and dissemination.

The end of unitary thinking

Knowledge without explanation: Postulated blunders of official science

Mathematics of reality vs. mathematical imitations of reality

As noted above, organisational, social and other “human” problems of modern science are directly related to the unitary, strongly and explicitly reduced framework of its content, and as the latter is determined

by the mathematical basis of science, one cannot avoid analysing its limitations that underlie and illustrate all higher-level features of knowledge. The *deliberate rejection* of a search for *realistic, complete explanation* of natural phenomena in terms of *natural* entities in favour of *purely abstract* “model” adjustment to *quantitative* results of *selective* measurements is the real, now totally dominating basis of official science doctrine, also known as “positivistic science” (due to explicit emphasis of the concept by Auguste Comte) and stemming from Isaac Newton's approach and attitude (“hypotheses non fingo”). It is that *very special*, strongly and *artificially* limited “paradigm” that was imposed on the *whole* body of knowledge since the apparent “great” success of Newton's model, which explains also its modern “end” accompanied by a catastrophically growing multitude of difficult, practically “unsolvable” problems and related “distrust of science”. Another kind of science oriented to the *causally complete* understanding of reality was founded by René Descartes half a century before Newton, but was later practically totally excluded from official science practice despite the accumulating difficulties of Newtonian “science without explanation”. That “terrible mistake” of conventional knowledge involves transformation of mathematical *tools* (or *language*) of science into its self-important, absolute and therefore dominating *purpose*, so that the whole tangible reality is finally replaced in that “positivistic” doctrine by purely abstract world of mathematical, imitative and immaterial structures (see e.g. [77-85]). But is it a problem of mathematics as such and can the unreduced, tangible reality be *exactly* reproduced by a mathematically rigorous language of science? As shown in detail in the universal science of complexity [42-44,66-76], the *positive* answer is due to essential and well-specified *extension* of the basically *wrong, artificially* restricted use of mathematics in conventional science.

Indeed, if we apply the same mathematical tools in their unreduced, truly correct version to the same natural phenomena, we immediately discover that the difficulties of usual science description are due to the following *major blunders* of conventional mathematics application arising from a *subjective* and artificially imposed bias towards maximum, unjustified, mechanistic *simplicity*.

(1) ***Uniqueness theorems***. Statements of standard mathematical theorems about *uniqueness* of generic problem solution, serving as a “well-established” basis for the whole scholar mathematics and its applications, are plainly and totally *erroneous*, just for generic, at least basically realistic cases of interaction problem (and for *all real* interactions). Indeed, as a rigorous problem analysis and its physically transparent interpretation convincingly show [42-44,66-76], every unreduced interaction process, underlying any observed phenomenon or system dynamics, gives rise to *dynamic multivaluedness*, or *redundance*, of problem solutions, where the problem actually has many locally “complete” and therefore *mutually incompatible* solutions called (system) *realisations* that are forced, therefore, by the driving interaction itself, to *permanently replace each other* in a *causally* (and *truly*) *random* (alias *chaotic*) order thus defined. Only unrealistically simplified, strictly one-dimensional (and timeless) interaction problem can have a unique solution. Usual mathematics incorrectly extends this situation to real cases by neglecting omnipresent real-problem instabilities with respect to multiple possible scattering ways implying multivaluedness of the effective interaction potential, as opposed to single-valued potential assumption (often tacitly) imposed in conventional theorems. Deeply related to this feature is a totally subjective inclination of usual mathematics for a basically *smooth* (unitary) and *closed* (“exact” or “separable”), i.e. again *simple*, form of all its

constructions, in striking contrast to the observed configuration and dynamics of natural systems.¹ In fact, usual, *dynamically single-valued* solution of a *real* problem does *not* even *exist* as such.

(2) **Self-identity postulate.** Irrespective of detailed statements and structures, a single, omnipresent, implicit postulate underlies all mathematical constructions of usual science, the “evident” assumption about self-identity, $\mathcal{A} = \mathcal{A}$, of any given structure \mathcal{A} (including its possible time dependence, $\mathcal{A}(t) = \mathcal{A}(t)$). The above dynamic multivaluedness and related permanent, unceasing realisation change of *any* system structure shows that real world objects and their *correct* mathematical images are *not* self-identical, $\mathcal{A} \neq \mathcal{A}$. Moreover, the same property of unreduced, multivalued system dynamics gives rise to the *rigorously*, universally defined notions of *event* (realisation change), *emergence* (new realisation formation) and *time*, $\mathcal{A} \neq \mathcal{A} \Rightarrow \mathcal{A}(t)$, the latter acquiring now its realistic, uneven, unstoppable and causally irreversible origin [42-44,67,69-71,73].

(3) **Absence of material quality.** Again irrespective of details, there is no possibility in usual mathematics applications to express the *tangible material quality*, or “texture”, of a real object as a whole, in its unreduced, directly perceived version. Usual mathematical “models” of reality can propose instead only immaterial, over-simplified, “ideal” images supposed to properly *imitate* real objects whose particular measured characteristics (such as density, roughness, etc.) can be expressed by respective quantities where necessary. However, one can never obtain anything closely resembling a real object as a whole, with its detailed structure and dynamics. In fact, usual mathematics is missing completely even such problem formulation. The causally complete framework of the universal science of complexity provides that missing *rigorous* description of material quality in the form of *dynamically multivalued* (and thus chaotically changing) *entanglement* of interacting system components organised, in addition, in a hierarchy of levels of *dynamically probabilistic fractal* [42,43,73,74] representing the *real* object structure.² Moreover, the same framework shows *why* one *cannot* obtain a correct quality expression in usual mathematics in principle: this is because its invariable simplification of real interaction process by artificial cutting of emerging dynamical links (in the search for an “exact”, closed-form solution within a version of “perturbation theory”) just automatically kills both key features of the unreduced solution, dynamic multivaluedness and fractal entanglement. In contrast to apparently desirable property of realism, official mathematics applications in modern science (dominated by so-called “mathematical physics”) seem to be *proud* of their immaterial nature *deliberately* favoured due to a very special assumption about the ultimate origin of the observed world structure [77-85], which reveals a strangely subjective, doctrinaire and almost “religious” attitude behind the

¹ One should emphasize the key difference of our *dynamic* multivaluedness from usual, mechanistic multivaluedness (also referred to as “multistability”) where “multivalued” solution components represent but an internal structure of a *single*, dynamically smooth (unitary) system realisation. Those components are *regularly* taken by the system along its *formally* advancing trajectory (or state evolution) in an *abstract*, rather than real, space (such as “phase space”, “state space”, etc.). All the “multiple”, *coexisting* “attractors” of scholar “nonlinear science” describe but usual, basically *linear* trajectory configurations in an abstract space, corresponding to the *unique* problem solution, though maybe having a very “intricate” and therefore “nonintegrable” detailed structure.

² Note that usual fractals do not possess the basic property of dynamic multivaluedness and are not obtained as exact solutions of respective dynamic equations (i.e. as a result of system interaction development). Therefore, they do not show crucially important features of dynamic entanglement of system components and unceasing chaotic change at all structural levels just underlying the correct expression of material quality by the dynamically probabilistic fractal.

allegedly “objective” form of knowledge officially supported and absolutely dominating in all “secular” educational and scientific institutions.

(4) ***Absence of genuine randomness.*** Dynamically single-valued, “exact”, or closed solutions of scholar mathematics do not leave any place for genuine *randomness*, which leads to multiple conceptual and practical difficulties with various related notions and ideas, such as *nonintegrability*, *nonseparability*, *noncomputability*, *chaos*, *uncertainty (indeterminacy)*, *undecidability*, broken symmetry, free will, time flow, etc. (see e.g. [80]). In connection with the above items (1) and (2), the universal science of complexity provides a *unified* solution to all those problems due to a clearly specified origin of *true* randomness in the *dynamically multivalued* structure of unreduced, *mathematically correct* problem solution [42-44,66-76]. This is important logical closure of the unreduced mathematics of real-world complexity, contrasting with growing “loss of certainty” of usual mathematics [87]: the realistic mathematics of complexity is quite certain and complete, transforming *any* problem into a *solvable* (integrable) one, but the obtained solution *structure* is as dynamically “fuzzy” (irregular) and diverse (changing) as real world phenomena.

(5) ***Continuity or false, mechanistic discreteness.*** Continuity, or homogeneity (unitarity) of conventional mathematical structures is a fundamental consequence of their dynamically single-valued, non-dynamic origin. In order to account for the observed uneven patterns, unitary theory introduces artificial, mechanistic discreteness showing the opposite, equally unreal limit of “infinitely sharp” ruptures. The unreduced, dynamically multivalued solution of a real problem reveals instead the phenomenon of *dynamic discreteness* (or *causal quantization*), where any system dynamics and resulting structures are obtained as continuous but *qualitatively uneven* sequences of system realisations and *transitions* between them occurring through system *reconfiguration* within its special, “intermediate” realisation [42,43,69]. In that way, one gets the causally quantised structure of physically real, tangible *space* and equally real, but immaterial, irreversibly flowing *time*. Many related, major structures and properties of unitary mathematics appear now as only rough (at best), basically wrong approximations to nonunitary structure of reality, including unitarity itself (e.g. of quantum dynamics), continuity *and* (mechanistic) discontinuity, calculus (including its “discrete” and “discontinuous” versions), evolution operators, symmetry operators, any unitary operators, Lyapunov exponents (and related chaoticity imitations), path integrals, statistical theories, etc.

It is important that while the fundamental problems (1)-(5) of conventional mathematics applications look different within its reduced framework, they acquire a *unified origin and solution* within the causally complete mathematical structure of dynamic complexity. That unification has an explicit manifestation in the fact that the whole world dynamics, in its unreduced, *totally realistic* version is presented now as a *single*, dynamically unified structure of *dynamically probabilistic fractal*, while all its properties, now *rigorously derived* and causally extending usual, mechanistically imposed laws and postulates, are unified by the *universal symmetry of complexity*[42-44,66,67].³ Correspondingly, the obtained results refer to *all* fields of

³ Universal symmetry of complexity provides a unified solution to another “eternal”, unsolvable problem of usual mathematics applications, that of systematic deviations of always “too irregular” real structures from “simple”, regularly structured symmetries of unitary theory. The latter then resorts to one of its characteristic “tricks” and introduces the idea of “spontaneously broken symmetry”, where one states that a symmetry in question does exist, but if it is not observed in real system structure and dynamics, this is because it is “spontaneously broken” (starting already from only one, “future” direction of real time flow, or entropy growth). That unitary “symmetry without symmetry”

knowledge, rather than only “exact” or “natural” sciences. Even though various knowledge fields remain *irreducibly separated* in usual science, mathematics is always viewed as a “universal language of science”. Universal science of complexity reveals the ultimate completion of that property, where the extended mathematics of complexity does provide a *causally complete* description of *any* system behaviour and ensures coherent transition between different systems and levels of complex world dynamics.

Purely abstract world: Fundamental physics without truth and the end of “unreasonable effectiveness of mathematics”

There is certainly a direct link between the strongly *limited*, evidently incorrect basis of usual mathematics applications in “exact” sciences (see the previous section) and its absolute *domination* in the *purely abstract* world picture used in conventional science. Indeed, all those “complicated details” of real world dynamics, which are boldly rejected (without any sound justification) in the standard science approach, just make the *essential* difference between abstract “models” of the latter and reality they pretend to describe. Starting with Newtonian “hypotheses non fingo”, that very special understanding of objective world description of usual, “positivistic” science doctrine finds its apparent confirmation by successful applications to properly selected, simple and “smooth” enough phenomena and structures. Even in those particular cases, however, there is a number of strong, persisting “mysteries” and “difficulties” often directly related to the real origin of entities and properties just deliberately omitted in mechanistic science (e.g. the origin of gravity, mass, equivalence between its gravitational and inertial manifestations, time and space in Newtonian mechanics, canonical “quantum mysteries” and relativity postulates in the “new physics”, etc.). There is no surprise that the proportion of “mysteries” grows catastrophically with ever more perfect observation results, where all those rejected “details” become visible and often constitute the “main effect”. At that stage, attained in science in the middle of the 20th century, the famous “unreasonable effectiveness of mathematics in the physical sciences” [87] (see also [84,85]) turns into even more impressive *ineffectiveness*, where the “advanced” unitary mathematics plays its increasingly esoteric games without any relation to real world structure (string theory, loop quantum gravity, etc.). However, it's enough to consider real interaction processes rigorously, without usual illegal “tricks” of perturbation theory, in order to obtain quite realistic, truly exact presentation of reality using formally the *same* mathematical tools (but now in a different, truly *correct way*, see items (1)-(5) in the previous section).

There is no surprise, therefore, that the characteristic “crisis in physics” giving rise to *officially* successful science revolution at the beginning of the 20th century is strangely reproduced today, after a hundred years of apparently quite prosperous application and development of the “new physics” results. Fundamentally incorrect, purely abstract “mathematical physics” artificially established as a single possible approach in science, despite its intrinsic “unsolvable problems” and persisting irrational “mysteries”, still cannot provide a truly sustainable science basis. The old doubts and search for a “genuine”, truly consistent

enters a sad list of other major “contradictions” of unitary doctrine, such as chaos without chaos and complexity without complexity (see also section 2.1.4 below). Universal science of complexity solves this symmetry problem by revealing the *irregular structure* of the real symmetry of nature, so that the symmetry of complexity remains always *exact* but it connects system configurations (realisations) which are *irregularly different* from one another.

science foundation reappear again and take now ever more “global” proportions, such as the widely acknowledged “crisis in cosmology” [88,89], intense multiplication of “invisible” entities (“hidden” space dimensions, “dark” energy and matter, “theoretically” needed but experimentally absent “supersymmetric” particle partners, etc.), or a deep impasse in the whole fundamental science development known as the “end of science” [3]. And while the leading priests of mathematical physics are still busily disputing the “absolute” advantages of their “personal” abstractions (e.g. [54,78]), it becomes increasingly evident that any, even most “elegant” mathematical trickery cannot solve the problem and the fundamental science development can be restarted and prosperously continue only after a decisive transition to the unreduced, totally realistic vision and description of reality.⁴

In the meanwhile, all the extremely costly experiments involving accelerators, satellites and other efforts of industrial scale are always based on those purely abstract concepts that explicitly fail to produce at least a generally consistent picture of reality. One can mention such purely mathematical, even theoretically disputable constructions as Higgs bosons, supersymmetric partners of “usual” particles, various candidates for “dark matter” particles and “dark energy” sources, gravitational waves, black holes, etc. It should be emphasized that those officially accepted (and uniquely supported) schemes show multiple, evident deficiencies already in theory and still they are used as a *single possible* basis for those huge experiments involving hundreds and thousands of highly qualified professionals.⁵ And when the performed expensive trials of the bankrupt concepts “prosperously” and inevitably fail, one after another (no found expected “superpartners” for ordinary particles, nor gravity modifications due to “hidden dimensions”, nor esoteric “dark matter” candidates, etc.), it changes nothing in the accepted practice and theories of scholar science: while personal incomes of failing enterprise chiefs continue to grow without limits, any reality-based, *causally complete* world description (e.g. [42,68-71]) is excluded from any support at all, despite its clear, though even unintentional, *confirmation* by the same experimental data [69].

However, even huge material losses and impossibility to initiate real problem solution can be not the most serious consequences of such “unlimited” deviation from elementary criteria of truth in science. As the purely empirical, technical science possibilities grow at a spectacular rate, their power exceeds now the *whole* range of natural structure complexity [42,43,69]. Correspondingly, arbitrary application of those empirical tools based on illusive mathematical structures and now multiply disproved postulate of their

⁴ As persisting problems of usual mathematics applications compromise the “unreasonable effectiveness” thesis in an ever more obvious way, an “additional” argument in their favour is advanced, that of intrinsic, “superior” *beauty* of pure mathematical constructions which *cannot* be useless in principle and therefore *should* find one or another reflexion in real system behaviour (e.g. [84,85]). That ill-defined beauty of *abstract* constructions was even proposed (allegedly by Paul Dirac) as an independent *criterion* of their validity for *real* world description. However, while mathematical physics applications degrade from canonical quantum mysteries to the totally self-absorbed modern branches of string theory or quantum gravity, their *abstract* “beauty” [78] advanced as the *only* remaining criterion of truth and actually appreciated by an *extremely narrow* group of devoted adherents, shows that probably the underlying aesthetical (let alone ethical) standards themselves are not as universal as it is usually assumed. At least one can see a direct contradiction between *that* kind of abstract aesthetical climax of *over-simplified* constructions and undeniable criteria of beauty of “usual”, directly perceived reality, where *more complex*, involved and variable objects appear as more beautiful ones (see also [42] for the rigorous substantiation of the last relation).

⁵ The failure itself of the dominating “standard model” of particle physics is interpreted as extraordinary possibilities of its development ... within the same model (see e.g. [90])! When they cannot deny any more the total fiasco of their “best possible” doctrine, official science leaders shamelessly transform it into another occasion to ask for new, heavy expenditures for experiments based on the discredited concepts. No limits for such kind of “science”, indeed!

“unreasonable effectiveness” is practically equivalent to the *premeditated destruction* of those *real* structures, with unpredictable consequences but guaranteed failure of “theory confirmation by experiment”. The omnipotent tsars of official science are well aware of the related dangers (see e.g. [47]), but they continue to impose their “old good” trial-and-error method beyond the well-specified limits of its applicability and any reasonable efficiency.

In that way, purely abstract structures can lead indeed to quite tangible, negative consequences for the real world they fail to describe but can effectively destroy. A part of that destruction already clearly appears in physics in the form of practically lost public interest and related lack of creative young researchers, which only amplifies the crisis of science content and its corrupt organisation practices. It's clearly a time for revolution: what else can reverse those deadly tendencies and transform the current deepening crisis into sustainable progress?

Quantum computers, nanotechnology, and other “applied” giga-frauds

Whereas growing difficulties of basic science acquire a fundamental, inevitable origin and consistent explanation (sections 2.1.1-2), it remains to hope that scholar research can have brighter perspectives in its more applied aspects, exemplified by recently appeared “hot” fields of quantum computation, nanobiotechnology, thermonuclear fusion revival, and various “computer science” applications, from new materials design to climate simulations. Closer examination of those trillion-worth new “advances” shows, however, that conventional science has quickly degraded from inconsistent imitations of reality to open “intellectual” fraud based on shamelessly “strong” promises that can never be realised, according to undeniable, multiply confirmed laws of the same science.

Thus, unitary *quantum computation* idea, consuming in the last years practically the whole volume of quantum physics and related research (it's enough to have a look at the paper list in quant-ph section of arXiv.org), provides a typical example of that strange combination of *strong* doubts about its practical realisation and ever growing publicity and investments into the extremely dubious enterprise. Indeed, even its active participants openly acknowledge that the most probable expected result of the whole activity is that full-scale quantum computers cannot be built [91]. There are numerous (but “strangely” ignored) particular doubts in fundamental quantum computer feasibility (see [43, section 2] and references therein, [92]). And finally there is a *causally complete* analysis of the universal science of complexity [43] that shows, within a *realistically extended* picture of quantum behaviour (including *genuine* quantum chaos), why exactly quantum computers cannot fulfil their promise even under most “ideal” conditions of their operation. It is easy to see that this *causally* substantiated conclusion simply confirms (and realistically *explains*) standard quantum postulates (as well as other fundamental laws, including entropy growth), which are already multiply confirmed experimentally and *contradict* the very idea of unitary quantum computation [43]. It is the scandalously abusive play on *supernatural* “quantum mysteries” of official “rigorous” science (“multiverse interpretations”, etc.) used now for invention of *real*, practically efficient devices that has permitted such incredible (and ever growing) deviation from elementary consistency and honesty. But why can such ultimately perverted activity continue in all the “best” scientific institutions and programmes? It can

simply because some officially “leading” scientists have their purely subjective and absolutely unbalanced preference for the underlying manipulation with abstract symbols and “fantastic” promises, while the “embedding” system of science organisation has neither real possibilities, nor interests necessary for critical limitation of such abuses (see section 2.2). Such is this another *real* result of the “unreasonable effectiveness of mathematics” (section 2.1.2).

A yet much larger modern science “bubble”, that of *nanotechnology*, is physically close to quantum computation case, but is actually based on a much less “scientific”, mainly publicity-driven trickery. Starting from the evident Feynman's blunder [93] about “plenty of room at the bottom” (directly contradicting major quantum laws), the nanotechnology affair quickly took the scale of unlimited science-fiction hype [94-96] that has received, however, a strangely generous and “top-level” support from all major sources [97]. However, similar to quantum computer case, none of the “fantastic” promises has led to a really novel result or application, despite many years of very intense efforts, “nice pictures” of “small, tricky structures” and continuing multi-billion investment (if only one avoids purely terminological tricks, very popular in this “prosperous” field, when e.g. former computer *micro*-chips are now *classified* as *nanotechnological* products, just because their details can be as small as a hundred nanometres). It finally becomes evident that the real, practical reason for that bizarre giga-fraud so easily accepted by the most prestigious institutions is the rapid shrinking of the formerly extremely large and prosperous field of solid-state physics (actually due to successful technological applications), whose adherents have found “nanotechnology” as an efficient replacement for their disappearing financial support. The story of that another “science without science” is especially disappointing because the truly scientific, fundamentally expressed and novel concept of nanotechnology (and related nano-bioscience) *does exist* as a particular application of the universal science of complexity [43,72,73], but is apparently lost on the background of superficial, money-driven publicity and deceptive successes of blind and dangerous empiricism.

A similar loss occurs in a yet larger field of *biological applications* of “exact” sciences, where their usual, unitary doctrine cannot explain the specific life properties even qualitatively, but proposes instead an infinite number of over-simplified mechanistic imitations of living system dynamics and rejects a realistic analysis providing unreduced life properties as manifestations of high enough levels of universal, interaction-driven dynamic complexity [42,43,73,74].

The same “sale” of nonexistent and improbable science results at a super-high price dominates in the field of controlled *thermo-nuclear fusion* for energy production that suffered from serious difficulties in a previous period (the end of the last century), but now has won a new, huge support (ITER project), despite the absence of practical progress or even theoretical solution. And here again, the real, underlying problem is due to irreducible dynamic complexity effects that just cannot be properly treated within the unitary science doctrine in principle. Not only strong and diverse plasma instabilities (due to the *genuine*, rather than simulative chaos) create particular difficulties in development of intrinsically inefficient hot fusion schemes, but much more efficient and promising approach of *cold fusion* can be formulated exclusively in terms of complex behaviour and therefore, not surprisingly, is either totally neglected, or pushed to a far margin of official science activity (on the background of multi-billion support for provably inefficient hot fusion).

Needless to recall, we deal here with not only practically appealing, but urgently needed application of global importance; and still the official science machine prefers to support its “best” (i.e. self-selected) *people* interests, rather than the objective *science* quest and related interests of humanity.

And finally, as if in order to definitely kill any remaining hope for occasional knowledge progress in the epoch of the end of science [3], the official science establishment gives a very strong support to a major “new science” imitation in the form of *computer (simulation) science* (see e.g. [98,99]) and its extremely vast scope of applications (“everything can be put in a computer” and simulated). Even apart from the evident fact that a “computer experiment” cannot provide in itself any additional understanding (while it is far less precise than real observation results and often simply unrealistic), the unreduced, multivalued dynamics analysis reveals a fundamental deficiency of such single-valued imitations (cf. section 2.1.1) prone to multiple instabilities and related arbitrary large deviations from real phenomena. A characteristic example of that glaring inefficiency of “computer science” is provided by various simulations of the “system Earth” behaviour in relation to growing ecological problems (e.g. [100]): after practically unlimited financial investments into the field one gets only the result that could be clearly expected from the beginning: the predicted “effect” is of the same order as the differences between various “supercomputer” simulation results, so that in the end one still can rely exclusively upon real-time system observations.

In all these cases, a logically strange but inherent property of the mechanistic science approach appears in its ultimately absurd form: official positivism imitates everything it can (or cannot!) using all accessible, ever more perfect *tools* of *purely empirical* technology, *irrespective of the obtained results utility* or any *real scientific* purpose of their production (now practically absent). It is *empirical tool technology* that becomes the purpose in itself. One deals here with infinitely multiplying and cycling circles of “trial-and-error” efforts looking “promising”, due to growing *technical* possibilities of new tools, but in reality dropping dramatically in efficiency down to practical zero because of the “exponentially huge”, practically infinite number of interaction possibilities within each “truly complex” (large enough) system dynamics [43,72-76]. The resulting deep impasse is evident: no progress is possible within the officially imposed science paradigm, and the more is the power (and cost!) of technical tools applied, the smaller is the hope to get out of the vicious circles of unitary thinking (cf. [48]). The epoch of blind empiricism is finished and it becomes really dangerous now, but still persists without practically visible limits, selfishly suppressing any attempt of provably efficient knowledge development. Only decisive, qualitatively big transition to the unreduced analysis of real, multivalued system dynamics can put an end to exponentially growing expenditures for successively failing, practically fraudulent giga-projects and open the urgently needed era of causally complete solutions to “difficult”, and now critically stagnating, problems.

True lie, or post-modern science: Unlimited complexity imitations

As even a quick glance at the official science landscape reveals a hierarchy of terrible deficiencies and critical stagnation of results outlined above, the ending unitary science System tries to preserve its absolute dominance (despite everything!) by making “concessions” to the evidently missing complexity in the form of mathematically incorrect, ultimately loose and speculative “science of complexity” without novelties, but

often presented as the necessary “new paradigm” (cf. [64]). Since in fact there is nothing fundamentally, scientifically new in that speculative “paradigm” remaining within the severely limited space of dynamically single-valued approach, the final result tends to the natural limiting point of the whole bankrupt science doctrine, the so-called “post-modern science”, made of arbitrary, senseless plays on pseudo-scientific words and symbols that not only have nothing to do with real system dynamics, but do not even pretend for it anymore, allowing for any evidently incorrect play with formal symbols and rules and considering purely verbal promises of a “new life” as the *only* desirable and possible result of scientific activity.

We shall not repeat here the detailed description of unitary imitations of complexity, their evident deficiency and catastrophic result of their artificial domination in all official science institutions. One can find those details in refs. [42-46] (accompanied by further elaboration in [66-76]) and a correct description of resulting “perplexity” in popular accounts [3,4]. It is enough to mention the underlying basic deficiency of unitary science analysis replacing the huge plurality of permanently changing realisations by only one, “averaged” (or arbitrary) realisation representing the single possible, unchangeable system version (see section 2.1.1). It means that *any* unitary science structure, *including* those from its *complexity imitations*, has *strictly zero* value of *unreduced*, universally defined dynamic complexity, one of the consequences being complete perplexity and disorder of multiple competing complexity definitions of unitary “science of complexity” (no one of them can ever have the necessary, universal properties). Of course, that omnipresent deficiency becomes much more evident in the analysis of “truly complex”, multi-component and characteristically “soft”, unpredictably changing systems culminating for living and intelligent ones (cf. [45]), but it is interesting that even such striking contradiction between reality and its “scientific” image does not stop unitary imitations of positivistic doctrine that doesn't see any problem in e.g. a living (and thinking) organism imitations by a sequence of smooth, fixed and totally abstract mathematical structures.

As a particular example, one can evoke the unitary science notion of “chaos without chaos” based on evidently incorrect extension of a local series expansion (“Lyapunov exponents”) leading to completely erroneous and conceptually flawed idea about real system evolution, instability and (false) “chaoticity”, shifting the origin of randomness to “poorly known” initial conditions. That “exponentially amplified” uncertainty would give rise to only false, rather than real randomness that would, in addition, depend on time, thus making system chaoticity a (strongly) time-dependent issue. In the case of *quantum* (Hamiltonian) system dynamics, that contradiction attains its paroxysm in the form of *inevitably* regular behaviour of expected chaotic systems, in (false) conflict with the correspondence principle, while the unreduced, dynamically multivalued interaction analysis provides explicit and consistent problem solution in the form of universal origin of genuine, time-independent dynamic randomness in quantum and classical systems (see [42,43,68] and references therein). Other cases of official chaos without randomness include various “chaotic” or “strange” attractors that always describe a *single* possible system trajectory in an *abstract* (phase) space that can only approach its eventual “equilibrium” state, without any true randomness. Genuine chaoticity origin is replaced here by incorrect Lyapunov exponent use accompanied by “intuitively” estimated computer simulation results that being a computer analogue of natural experiments may reproduce, in principle, an unpredictable part of real system chaoticity but without revealing its true origin and

characteristics. After which such “well-established” chaoticity doctrine is applied to a huge variety of systems, from few-body interactions to social dynamics, intelligence, and ecological systems.

And because such arbitrary deviations from elementary consistency are not only possible but dominating in the official science practice, it becomes also possible and quite “natural” to pass to *explicitly* and *officially* unlimited deviations, in the form of pure play on words of open post-modern science that doesn't need any more even those false justifications and derivations of “rigorous” unitary science and can make arbitrary guesses based on borrowed “tricky” terminology, without any understanding (let alone direct verification) of its meaning and origin (see e.g. [23-25]). Those post-modern games of ultimate knowledge destruction often hide themselves behind a superficial demand of “interdisciplinarity” (e.g. [65]) based on a “felt” necessity to transgress traditional disciplinary boundaries but actually reduced to *unlimited inconsistency* and arbitrary application of any “model” or notion to any phenomenon.⁶ As the intrinsically complete interaction analysis of the universal science of complexity clearly shows, conventional disciplinary ruptures directly originate in the severely limited picture of *zero-dimensional reality projection* of the unitary science, while the full, *dynamically multivalued* vision of reality has no such problem in principle and provides “coherent”, logically correct transitions between “disciplinary” views extended to *levels of complexity* of the unreduced nature dynamics as confirmed by very diverse applications [42-45,66-76]. However, as official science sticks to the above over-simplified imitations of complexity, one obtains as a result not the promised unified, interdisciplinary knowledge (and *harmonious* society based on *such* knowledge), but rather knowledge and society based on the generally accepted, publicly supported lie and fraudulent imitations without limit.

Doctrinaire science organisation: The curse of unitary paradigm

As follows from previous sections 2.1.1-4, the official science system supports exclusively the evidently inefficient approach of the unitary (dynamically single-valued), or “mechanistic”, paradigm, despite quite explicit and quickly growing dangers from such science practice for the scientific enterprise itself and the related technical, economical, and social development in the whole. Because of blind, but technically ultimately high and therefore destructive power of unitary science, hitting today the *whole scale* of natural system complexity, the very survival (let alone sustainable progress) of human species after such “experimentation” becomes a more and more questionable issue (section 2.1.2) clearly felt as such by official science leaders [47]. If all of it is so clear, then why does the self-destructive enterprise continue so prosperously its activity profiting from a US\$ trillion-scale, always growing yearly support? The deeply rooted, intrinsic limitations of *unitary thinking* trying to mechanistically simplify everything it touches upon is one general reason for it.

⁶ Thus, any limited mathematical tool or model can boldly and directly be applied to description of any higher-complexity phenomenon (such as life, intelligence, consciousness), as if its authors ignore the evident and essential difference between the tool simplicity and complexity of real structure it is applied to (see e.g. [101,102] for only one recent series of the kind). Nevertheless, it is those explicit and terribly incorrect *imitations* of complexity (often obtained by direct, and always silent, simplification of its unreduced description, e.g. [42,43,69,74]) that obtain generous support of the official science establishment.

A related, more practically rooted reason for the persisting domination of the suicidal doctrine is its *organisation* in a *Unitary System* that naturally reproduces in its centralised hierarchy the glaring defects of unitary knowledge content: mechanistic simplification, rigidity, ruptures and totalitarian stagnation of development (in other words, such science, such organisation of science). That *effectively* centralised, *administrative* kind of human activity organisation has proven its total inefficiency in general social life organisation by the recent spectacular fall of the command economic and social system in the Communist Block of countries (Soviet Union and its satellites). But exactly the same kind of system still strangely dominates in organisation of science in any country, where it is realised by the world *intellectual elite* than should at least understand the evident fact, if not the origin, of the administrative system failure. By analogy with the last phase of Communist regime existence, such strange “inertia” reveals the ultimately deep level of *corruption* within the Unitary System of science organisation, where superior hierarchy layers are paid enough (actually by themselves!) just in order to preserve the “nourishing” System, irrespective of its production efficiency (which is an example of *negative* “self-organisation” reduced here to dirty *privatization of truth*). Various versions of strong, dominating corruption at all levels of modern science, varying from open “intellectual” slavery to explicit or implicit scientific fraud and plagiarism, are extensively described in various sources and publications [1-46],⁷ and still there is no visible change of tendency or at least attempt of science organisation reform, which demonstrates again the huge negative power of that *practically implemented* mode of unitary, positivistic thinking as well as the necessity of a *unified, revolutionary* kind of change in *both* scientific knowledge content and organisation.

It is rather evident that the only reasonable kind of such revolutionary change is the unified transition from unitary to realistic science content (exemplified by the universal science of complexity) and from administrative to free-production, creative and individually structured science organisation [42-44,46] (see section 3 below for more details). Just as modern, unitary science structure closely corresponds to its mechanistic content, the free-interaction kind of science organisation at the forthcoming higher level is directly related to the new science content (unreduced complexity of real system dynamics), so that the new science of complexity can be successfully applied to (consistent) understanding of science development itself (which can never be the case for unitary science content and development). Another feature confirming not only the necessity but also reality of deep science organisation change is the possibility to perform it starting from *small enough volumes* of a big enough change (as it happens in higher-order phase transitions in physical systems, contrary to the whole volume change in the case of first-order transition).

Correspondingly, there is no sense to try to increase science efficiency by “ameliorating” its organisation details without changing the system as such (this kind of proposition inevitably dominates in presented official science ideas about its own reform). It is at this point that *rigorously substantiated* and *causally complete* results of the universal science of complexity on such “truly complex” structure evolution provide the *uniquely reliable* basis for practically important actions.

⁷ Beyond any public recognition, private communication with professional colleagues, which is especially efficient for such “contradictory” issue discussion, shows convincingly that published information demonstrates only “the tip of the iceberg” of science organisation and practice problems.

Science of lie: The ultimate deadlock of scholastic knowledge

As a result of presented analysis of official, positivistic science paradigm (in both its content and organisation), sections 2.1-2, one can see that the clearly perceived modern change of scientific knowledge (e.g. [1-3,23-27,47-58]) should be specified as indeed the ultimately deep, qualitative and “final” crisis of the whole existing kind of fundamental science, i.e. the true end of *unitary* science, and the closely related transition to superior kind of knowledge (see also section 3 below).

Major features of usual, unitary science basis, viewed now from the new, causally complete knowledge perspective (section 2.1.1), reveal fundamental, inevitable, and *rigorously specified* limitations of that *particular*, very special kind of knowledge in the form of its *effectively zero-dimensional* (dynamically single-valued) vision of all studied objects and phenomena, which is the real, concrete reason for the now exhausted possibilities of its further development and resulting impasse, or “end”, of science. The necessity and origin of the new, causally complete and totally realistic kind of knowledge based on the *truly exact* analysis of *unreduced*, dynamically multivalued behaviour of real systems becomes equally clear, thus completing the emerging picture of objective relation between real world structure and its reflection in human knowledge.

The ultimately big scale and importance of the difference between usual (unitary and fixed) and new (realistic and creative) kinds of knowledge should not be either underestimated theoretically (as being due only to “knowledge refinement” or superficial “interdisciplinarity”) or overestimated practically (as impossibility of further science development at a *superior* level, cf. [3]). The huge scale of modern science impasse and related transition to superior kind of knowledge is emphasized by the fact that today's change terminates scholar (and *scholastic*!) science development during its *whole history* of more than three centuries, starting from Newton's paradigm of a “technically adequate” description that does *not* need additional, complete *explanation* in principle (“hypotheses non fingo”). It is precisely that, very special science content, practice and attitude later summarised in the form of *positivistic science* doctrine that reaches now the ultimately deep, final impasse and contradiction to the necessity of genuine understanding of real, arbitrary complex system dynamics.

One can easily understand the origin of major observed properties of thus specified unitary science, its *scholastic dogmatism* and dramatic degradation from “plausible”, technically correct imitations (“models”) of reality to “scientifically packed” but obvious *lie*, i.e. *arbitrarily large* separation between “models” and real structures they are supposed to describe.

Dogmatism results from the dynamically single-valued, over-simplified “projection” of reality within unitary science paradigm that does not leave any place for a flexible interplay between multiple, often “opposite” system properties readily observed in nature (recall e.g. “wave-particle duality” observed already at the lowest levels of real world dynamics and remaining mysteriously inexplicable in the allegedly “rigorous” approach of official science). By contrast, such naturally occurring interplay constitutes a major, intrinsic property of unreduced, dynamically multivalued system behaviour in the universal science of complexity [42-44,66-76]. It also gives rise to *dynamic complexity development* from its lower to higher

levels that provides a universal and causally complete solution to the problem of “interdisciplinary” vision and “reduction” of higher-level properties to lower-level interactions (or dynamically *emergent* system properties). There can be no more place here for a dogmatic fixation on a particular system property, quality, or feature.

In a similar way, the effectively zero-dimensional, point-like reality projection of unitary science cannot avoid *arbitrary large deformations* with respect to the unreduced, “multi-dimensional” (dynamically multivalued) structure of reality, where such deformations naturally grow from simpler (more fundamental) to higher-complexity systems (which explains an otherwise “strange” combination of a *relative* success of unitary paradigm application to simple physical systems and its catastrophically growing failure to provide an equally “exact” description of higher-complexity systems). Such surrealistically big deformations of reality, multiplied by their exponentially growing number and totally abstract origin create today the situation of “science of lie”, i.e. a form of allegedly “objective” knowledge but where the criteria and very notion of truth become “infinitely” smeared (“everything is possible”) [3,4,23-25,27,28]. But as that “post-modern” state of “life after death” in science is accompanied by a particularly prosperous development of *empirically based* technology, one obtains a yet more surrealist situation where that ultimately wrong and practically dangerous science of lie is taken as the uniquely suitable basis for the expected “knowledge-based society”. One should obtain thus a very “futuristic” society based on lie! (It may already be the case, looking at modern tendencies in “developed” countries...)

The transition from apparently prosperous, though maybe “non-ideal” fundamental science to its modern version of ultimately uncertain “fairy tales” with no relation to reality had *silently* happened somewhere in the middle of the 20th century, while the official science *status* has remained at its highest peak till recently by simple “inertia” effects of habitual propaganda of “evident” advantages of knowledge and its progress. In the meanwhile, another, *purely empirical* development of *technological tools* of science has attained equally critical (and also hardly recognised) point just in the same epoch, where they could, for the first time in human history, directly touch and modify the *deepest levels* of natural system complexity, from elementary particles to biological and ecological (as well as social) systems. In that way, a tacit, but catastrophic drop of understanding of real system structure and dynamics coincided with the equally dramatic (and unrecognised) increase of the purely empirical power to change them *completely*. It does not seem difficult to see that the *cumulative effect* of those two major changes acting in the *same direction* leads to a *major transition*, in the middle of the 20th century, from generally, externally useful (and therefore also relatively consistent, prosperous, creative) science to its ultimately dangerous, practically dead, and critically decaying today's version, even though one deals in both cases with exactly the same, positivistic, unitary, mechanistic science doctrine. Clear recognition of that crucially important transition at least now, when the risk to suffer from its negative consequences is at its maximum, is a necessary condition for any further knowledge and civilisation development that can only take the form of revolutionary transition to a superior kind of knowledge (section 3).

As pointed out above (section 2.2), the observed sharp crisis of official science practice and organisation is closely related to the problem of its unitary content, so that both scientific knowledge content and

organisation can break the current deadlock and start up a new, unlimited progress stage by the unified transition to the unreduced vision and analysis of real world phenomena as exemplified by the already realised framework of the universal science of complexity [42-46,66-76].

From dogmatic to creative knowledge: Revolution of complexity

Science of truth: Intrinsically complete knowledge

It is not difficult to see why further science development is impossible without a crucial progress towards genuine understanding of real interaction processes: the latter dominate the whole scope of modern practical applications where both intensity and “interconnectedness” of various interactions grow now without visible limits (it is a widely understood *globalisation* phenomenon). But as it is universally demonstrated in the unreduced complexity analysis [42-44,66,67,75,76], such real, arbitrarily large and intense interaction processes always give rise to rigorously specified dynamic complexity, in the form of permanent realisation change in a truly chaotic order. It becomes evident, therefore, that the highly needed and even *empirically*, tentatively emerging knowledge revolution mentioned above can only involve transition from the now dominating unitary, dynamically single-valued (zero-complexity) science and thinking to the unreduced, dynamically multivalued, or complex, behaviour of real systems and its qualitatively new analysis. The emerging summary of ever growing number of “unsolvable” fundamental problems (sections 2.1.1-4) just directly demonstrates those “missing dimensions” of the official science framework. In other words, the forthcoming knowledge revolution is clearly specified now as the *revolution of complexity* understood as a *practical transition to superior complexity level* not only in formal, scholar knowledge, but also in various applications and general attitudes. Needless to say, such complexity revolution is very far from its bubbling unitary imitations that cannot provide even a clear definition of the main quantity of interest, complexity itself [3,4], let alone solve accumulating real problems. Talking very loudly about “the century of complexity”, unitary science priests, provided with maximum material support in countless “advanced” institutions, in practice only submerge ever deeper in over-simplified, totally abstract games with mathematical structures of *zero complexity* (see sections 2.1.1-4), which clearly demonstrates their real intentions and capacities.

Despite that purely subjective suppression of the unreduced complexity by the whole official science establishment, its triumphant advent is inevitable at the progressive development branch, with the only possible alternative of destructive “death branch” [44]. Clear signs of the latter are already visible in the content and practice of official science (sections 2.1-3) just because it resists obstinately to the progressive tendency. In the meanwhile, one can objectively estimate, already now, the main properties of the forthcoming new knowledge and preceding complexity revolution.

Just as severely limited projections of unitary science lead inevitably to what can be called, due to essential and irreducible separation from reality, science of lie (section 2.3), the intrinsically complete knowledge of the unreduced science of complexity leads to the *science of truth* containing no ambiguous,

abstract “models” and criteria of their validity varying subjectively in proportion to a personal “push” of one or another “eminent” scientist. The absolute, *objective criterion of truth* of that new, unreduced form of knowledge is its *total consistency*, i.e. absence of any noticeable contradictions in the *entire system of correlations* within the available (in principle, growing) volume of knowledge [42]. It is very different from the situation with irreducibly separated “models” of unitary science where each model (a point-like projection) can reproduce some system properties but fails with other, equally important ones (e.g. “wave-particle duality”, or in general “complementarity”, in particle physics). Another, quantitative expression of the same criterion of truth states simply that unreduced complexity of “mental” constructions of knowledge (i.e. our “understanding” of reality) should be *equal* to that of described real objects (including brain-reality connections as they represent but a particular case of complex-dynamic interaction). In other words, science of truth provides, in agreement with its name, the *truly exact* version of the world structure and dynamics (within a growing volume of empirical data), being thus the *intrinsically complete* (totally realistic and consistent) form of knowledge. By contrast, as follows from the same criterion, unitary science knowledge represents the *largest possible* deviation from truth as it has the minimum possible, zero value of unreduced complexity (while real structure complexity is always high, even for the simplest structures [42,69-71]). This is the real reason for a strange combination of visible, mainly empirical “successes” of positivistic science and its *growing* failure to *explain* even the simplest, most fundamental observed entities and properties (those of time and space, elementary particles, etc.).

Because of that special nature of new knowledge and its difference from the previous level of unitary science, the transition to the unreduced complexity science also has quite special properties of the *last scientific revolution* [44]. According to the well-known idea of scientific revolution, or “paradigm change”, by Thomas Kuhn [62], the process of knowledge acquisition has a quite uneven and very “contradictory” structure involving antagonistic fight between emerging new ideas and inertia of currently “established” knowledge. Although the nonuniform character of any progress is confirmed by the causally complete analysis of the universal science of complexity [44], it becomes also evident that unitary science “progress” has been occurring mainly in the form of *empirical discoveries* due to *technical* progress of research *instruments*, while the essential *understanding* of reality has always remained at the same superficial, lowest possible, zero-complexity level. This is exactly why the change to a new paradigm within unitary science has that “antagonistic” character: without genuine understanding of reality, each new discovery appears as a “miracle” difficult to believe in at the beginning, until everybody can empirically verify its reality (recall microbial disease origin discovery by Pasteur, difficult emergence of the “new physics”, etc.). But the discovered “new” reality is *not* consistently explained either, usually it would even be *less* understandable than previously known “simpler” objects, which endows unitary science “progress” with a “contradictory” property of *growing accumulation of mysteries and unsolved problems*, while even old, previous-level problems do *not* really obtain their clarification after the advent of a new paradigm (postulated Newton's laws and “classical” properties were not provided with any deeper understanding after the appearance of the “new physics” that had instead brought about its own mysteries growing in number and scale until now!).

One can see now why the forthcoming transition from unitary science to the unreduced science of

complexity, or revolution of complexity, has quite a different character with respect to Kuhnian scientific revolutions and gives rise to a *qualitatively new kind of knowledge progress*, in the form of *permanent extension* of previous *understanding* of reality (*growing consistency* rather than mystification), without those painful ruptures and antagonistic contradictions between old and new knowledge. It is the expression of the *intrinsically creative* nature of the *causally complete* knowledge of unreduced complexity science. After transition to that superior kind of knowledge, Kuhnian scientific revolutions become impossible as such: uneven knowledge progress proceeds without ruptures and inexplicable “miracles”. That's why the revolution of complexity is the *last scientific revolution* or, more precisely, it puts an end to unitary scientific revolutions and opens a qualitatively *new era* of *intrinsically sustainable, progressive development* of a *new kind* of knowledge.

It is interesting that the basis for that new kind of knowledge has actually been created by René Descartes yet before the appearance of Newtonian, positivistic science that “does not invent hypotheses” (i.e. explanations). Indeed, it is easy to see [42] that Cartesian science (e.g. [103,104]) was just very strongly oriented to production of the *most complete explanation* of observed phenomena (which even sometimes led Descartes to erroneous theories, in the absence of necessary experimental data). Related Cartesian approach *creativity* (due to his famous concept of constructive *doubt* [103]) and *unified* (“interdisciplinary”) character of resulting knowledge only confirm similarity with the unreduced complexity science and the fact that such another kind of knowledge was initiated before Newtonian positivism, at the very beginning of modern science as such. Newton later argued in favour of his apparently exact (and therefore visibly efficient) *quantitative*, mathematical analysis, which he absolutised to the idea that one would even *never* need *anything else* than “exact” mathematical models, any additional explanations being but useless “interpretations”. Whereas Newtonian science application to (major) planetary orbit description may seem to justify such attitude, its failure to solve already a three-body problem, let alone provide an adequate description of “truly complex”, e.g. biological, systems shows convincingly that “everything is not so simple” in the world containing a lot more than combination of regular Platonic figures of the unitary paradigm (cf. [84,85]). The problem was, therefore, not so much in that Newton had strongly imposed his indeed apparently “perfect” (at that time!) theory, but that it was not later properly completed and extended again to the lost consistency of initial, Cartesian science paradigm, not even at the moment of *explicit complexity emergence* in the “new physics” discoveries at the beginning of the 20th century (chaotic dynamics, stochasticity, quantum and relativistic behaviour, cf. [42,69]). It is only today that one can see the true meaning of the lost “method” of René Descartes, and even now, almost four centuries after his work, the huge machine of official science, provided with “miraculous” technical power, continues to operate senselessly for the evidently fruitless mechanistic doctrine unable to ascend to the level of intrinsically complete knowledge founded by Descartes at the dawn of scientific age.⁸

⁸ Even worse, the bankrupt unitary science tends to attribute to Descartes a particular attachment to its own, mechanistic approach, while reserving to itself the honour of “extension” of such simplified vision! It also tends to see Descartes as a “mere philosopher”, rather than scientist, despite the well-known fact that he practically elaborated the mathematical basis of science itself, as well as much of “Newtonian” motion laws and various other results confirmed later, while being strongly restricted, at his time, by few experimental data and possibilities. That is another instructive lesson of knowledge development where unitary science of lie confirms, once again, its major character and purpose. It is

That “contradictory” way of real knowledge development shows that it is far from being finished and the unreduced, realistic science has not even started yet its genuine, unrestricted progress, remaining under the pressure of *purely subjective prejudice* of a pre-scientific epoch of *scholastic*, religion-based kind of knowledge (superficial *interpretation* of a *basically fixed* doctrine based on a *blind belief*). New knowledge of the unreduced science of complexity, being modern realisation and well-specified renaissance of anti-scholastic, Cartesian tradition, is a *permanently developing*, open kind of knowledge devoid of artificial, subjective limitations (such as the “necessary” mystification of modern “rigorous” science) and guided only by the universal criterion of truth, the *totally consistent understanding* of reality within the whole accessible, ever growing volume of knowledge.

New science organisation: Interaction-driven creation

It is evident that the forthcoming superior kind of knowledge needs a new organisation, which should be as different from today's official science establishment (section 2.2) as the new science content (and real world dynamics) is different from the unitary knowledge projection. The intrinsically creative organisation of new knowledge can only be realised in the form of a free-interaction system of quasi-independent units of knowledge creation, support and propagation [46]. Those *independent science enterprises* would mainly be based on *individual* researcher efforts and small teams, but being freely composed and recomposed structures, will certainly include various hierarchical unifications (e.g. within particular projects), as well as accompanying *knowledge management enterprises* that will make their business on optimal financial support for *knowledge creation enterprises* and their result application, dissemination, and public estimation.

That transition from today's unitary, rigidly centralised and administrative science organisation to the superior system of freely, constructively interacting knowledge creation units is analogous to the transition from a command economy within a totalitarian political system to a (regulated) free-market kind of economical life, with similar advantages in creative power and efficiency. There will be no more formal, unnecessary, subjectively driven limitations for either scientific creation itself or science organisation development that now becomes an *integral part* of creative knowledge production [46] instead of modern inefficient and corrupt bureaucracy serving just to *limit* knowledge development in favour of selfish interests of reigning feudal priests of science and their perverted clans. The acute crisis of modern science [1-58] widely recognised even within its unitary “establishment” shows convincingly that this kind of transition in knowledge organisation is more than urgent, if any knowledge progress is to be preserved at all. Only a free-interaction kind of development can provide a truly efficient science organisation that becomes vitally necessary today for the whole civilisation progress, especially taking into account the huge and ever growing role of powerful modern technologies that easily become frustrated and dangerous without a guiding contribution from a constructive, problem-solving and *independent* (i.e. truly free) fundamental research (section 2.3). That is the only possible solution to a stagnating and much discussed problem of the *freedom of*

interesting that Descartes prodigiously foresaw even that sad destiny of his approach when he wrote that many centuries should pass before science would be able to properly develop the principles of his approach.

scientific research (see e.g. [60,61]), which cannot be solved by any “reform” of traditional, unitary organisation of science. Indeed, contrary to naturally established practices of a free-market system, inefficient administrative “research” without any real progress can (and does!) continue for many decades without even any attempt to change its leaders and content.

Similar to higher-order phase transitions, that “symmetry change” in science organisation can occur gradually by its volume, but remaining qualitatively strong there where it does happen. A qualitatively big transition becomes thus more practically feasible than a simultaneous transition in the whole volume (first-kind phase transitions). Starting there where it is more probable and necessary, the transition to “distributed” and intrinsically creative science organisation will grow by its natural success and include interaction patterns and enterprises of various suitable scale and function. As there is no reasonable limit to such kind of intrinsically creative development of science content and organisation, one can be sure about its sustainability that doesn't need any additional, centralised kind of administration, as opposed to any version of unitary organisation. Note that first attempts to create interactive, quasi-independent units of research of a new, “liberal” kind already appear and grow everywhere, even though they still rely essentially upon usual science organisation.

Finally, it is worthy of mentioning that such transition to a superior, qualitatively more efficient kind of science organisation can serve as a prototype of a similar, equally necessary transition in the whole social structure suffering today from a deep development crisis [44].

Sustainable development based on causally complete knowledge

While the necessity of transition to another, qualitatively different kind of civilisation development becomes evident from various perspectives and within different approaches giving rise, in particular, to the ecologically motivated *sustainable development* idea [100], it seems yet to be poorly recognised that such important change can only be based on the equally deep progress of underlying knowledge, so that the desired truly sustainable civilisation development can be *uniquely* realised in the form of society based essentially on a *new kind of science* ensuring *causally complete, totally consistent understanding of all practically modified systems* [44]. As the already realised applications of universal science of complexity convincingly demonstrate [42-44,66-76], that kind of knowledge uniquely provides the causally complete understanding of fundamental universe structure, laws and purpose, from elementary particles and cosmological problems to life, intelligence, consciousness and their development. It is such unreduced, reality-based vision of the forthcoming change of science that should guide its modern development, as opposed to obscure manipulations of unitary science scribes remaining enclosed in their self-interested, narrow doctrines and abstract models separated from real life and related purposes of human progress.

The best science advances have always been driven by intrinsic, individual creativity and constructive interaction within the whole civilisation development. But those could only be rare, “enlightenment” moments in the dominating kingdom of scholastic unitary thinking. And in today's epoch of “material life” triumph, fundamental knowledge as such has lost its creative character, superior purposes and has become

just an imitative, parasitic and unpopular appendage to flourishing empirical technologies. There is no positive solution on that way of quickly advancing decadence, for either science or civilisation whose development it should guide. Any hope for usual, evolutionary progress by small steps within the existing system is vain, that is the definite conclusion of both rigorous analysis of the universal science of complexity (applied now to the system of science or civilisation as a whole) and accompanying qualitative considerations. The last scientific revolution outlined in this paper is a unified and uniquely consistent change to another, progressive branch of development of knowledge and civilisation based on the unlimited power of that qualitatively new kind of knowledge.

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What is research?¹

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I am going to take a critical look at what it means to do research. The point of view I express is a personal one that seeks to disturb the calm and complacent consciences of those who are dedicated to research. You may not agree with me, but I hope that at least you will take what I say as an invitation to reflect and perhaps even formulate fresher points of view.

It all began when our primary or secondary school teachers inspired us by telling stories about the struggles and miracles of science. This did not impress all the pupils, but it did some of us—after all, here we are doing research. Our ability to solve problems that the other kids found difficult was a factor that sharpened our interest in the sciences. We felt a kinship with science and mathematics and experienced a certain ego-boosting pride, as if to say, “here I count for something”.

As lovers of scientific knowledge, simply thinking about the great events in scientific history was enough to feed our intellects. We were told of the exploits of Galileo, Newton, Darwin, Einstein or Bohr, who for us became heroes worthy of emulation.

Eventually, we finished our degrees with high grades and were able at last to gain access to one of those 'high-tech' centers where, so we were told, research is done. “But what exactly is it that we really do?”

History teaches us not to separate individual experience from general events. Hence, when Galileo observed the satellites of Jupiter, he was also demonstrating that not everything revolves around the Earth. Newton's laws were not formulated for the purposes of engineering applications but to reveal the non-teleological mechanism of our Universe. When Laplace told Napoleon that he had solved the system of equations which explain the motion of the planets without the need to invoke God, the important point was not his mathematical juggles but the struggle to arrive at the truth without resorting to ancient mythologies. Darwin put humankind in its place within the animal kingdom. Etc.

Changing ideas about the Universe are what drives the scientist. Before becoming a data collection on Nature, science was mainly devoted to combating superstition, the principal aim was rigorously to realize the dreams of Epicurus and Lucretius—to overthrow the idea of the gods controlling the Universe, to emancipate Nature from the grip of haughty lords and dark, mysterious forces, to demystify the Universe and face truth

¹ Modified version of the text published originally in *Metaphysical Review*, 4(2), 5 (1997), an electronic journal which has disappeared.

head on. In other words, knowledge for the sake of knowledge, the elevation of mankind with an understanding of his surrounding without need of resorting to white lies.

We live in different times. Nowadays oppression does not come from powers making claims on behalf of divinity. The value that motivates today's world is called Money rather than God. The conspiracy between capitalism and democracy is all-consuming, their enemies have two destinies: either be absorbed or be eliminated. The applied sciences have always been allied to capitalism; they drive technology and flood the market with products labelled with a price. The pure sciences, or those with non-industrial applications, such as astronomical research, for the most part, were revised in terms of their driving principles; they were adapted and absorbed to the needs of our times. Present-day utilitarianism revolts at the idea of knowledge for its own sake. Even Buddhism, with its initially antimaterialist ideas, has been rendered into a marketable product in the book shops or in the form of courses on transcendental meditation. Culture has also been turned into a "cultural industry", to use Adorno and Horkheimer's expression. Scientific knowledge has become a milk cow on which to grow fat², an industry providing jobs to some State employees in order to make possible they can live with their spouses and their children in the welfare state.

Genius science is substituted for science of the masses and for a democratic science that advances with the rhythm of mediocrity. The stomach is put before the brain. Everything is bureaucratized, everything requires paperwork and to conform to mediocrity in order to effect a project.

"Democratic resentment denies that there can be anything that can't be seen by everybody; in the democratic academy truth is subject to public verification; truth is what any fool can see. This is what is meant by the so-called scientific method: so-called science is the attempt to democratize knowledge - the attempt to substitute method for insight, mediocrity for genius, by getting a standard operating procedure. The great equalizers dispensed by the scientific method are the tools, those analytical tools. The miracle of genius is replaced by the standardization mechanism." (Norman O. Brown, "Love's body")

Those research ideals have been left behind. Intellectual restlessness, the search for truth created those colossi of knowledge who moved among the different fields like salmon among rapids. Today, such pirouettes have become impossible because knowledge has become heavy and sluggish. You will see an elephant sliding before you see a scientist knowing so many fields as our scientific forefathers did. Nowadays, a scientist has to specialize. Scientists have been specializing for quite a long time, but it is now a question of microspecialization. There are experts on cool stars, the Galactic bar, certain types of chemical reactions, etc. The most a scientist can hope to achieve is mastery of a few microspecializations, in which to invest their efforts or creative interests. It is hard to imagine someone getting into a specialization because it is his only interest, unless the system has sent him crazy enough to believe that his topic is the centre of the world. This clearly is not so. Rather, it is more a case of converting the scientific process into an industrialized mass-production system. Everybody attends to his own cog so that the system runs smoothly.

² "Science. Heavenly goddess for some people, and an industrious cow which produces butter for other people." (Goethe)

It is a treason to our scientific forefathers' ideals. Descartes gave science a sense for mankind as a search of truth in his “Rules for the direction of the mind”, and expressed in the first rule:

“Thus, if somebody wants seriously to research the truth of the things, must not choose a peculiar science, since all of them are related among themselves; rather, he must only think about increasing the natural light of reason, not in order to solve this or that school difficulty but to get an understanding about life that shows us the behaviour we have to choose.”

And, what does the scientific industry produce? The answer depends on the observer. From inside, we see tons of printed paper which is not read by anybody except some few specialists, each one about his topic. From outside we get a hermetic impression, such as we said “what amazing things these people must be discovering! It must be so difficult and advanced that it not accessible to my level of understanding”. That is the impression which is produced among those who pay the taxes so the State will continue funding further research. Those who are dedicated to applied sciences have an easiest task because they promise technological advances. The pure sciences without immediate practical applications, in order not to loose the thread of State subventions, must also promise technological progress for the country in the long-term. If it is necessary, they lie. If the technological argument does not work, they attempt to impress people with the knowledge content. If it is necessary, they exaggerate. They say that a satellite or a telescope is going to create a revolution in astronomy, that we are going to observe the whole Universe and some parts of other ones,... and then the artefact arrives and... the revolution has been rather small. Perhaps they scrape something else about some galaxy which was not in our collection.

We must not deceive ourselves. The more the history advances, the more difficult the achievement of a relevant truth is. Newton's scientific activities during one year of his life, with a mere notebook and a pen, were more fruitful than the activities of thousands of the best actual scientists in their whole lifetime and with millions of euros... It seems that there are many writings, many data,... but in the final analysis our comprehension of Nature in global terms advances at a very slow and nearly imperceptible pace. Great efforts bear less fruit.

“Death of science consists of the existence of nobody able to live it. But 200 years of scientific orgies get fed up in the end. It is not the individual but the spirit of a culture who gets fed up. And this is manifest by sending to the historical world of nowadays³ researchers who are more and more small, mean, narrow and infecund.” (Oswald Spengler, “The Decline of the West”)

The fight for the economic power and social status promote fights among specialist from different fields rather than searching “the truth” altogether. Astronomers ask money because they are disembowelling the cosmos secrets; the particle physicists are disembowelling the matter secrets; the biologist the life secrets; ... What impatient individuals who want to reveal all of Nature's mysteries and do not want to leave anything for the next generations! Some data has not still been exploited completely and we think about getting the next data. Fast!, before any other makes the discovery! Impatience has never been typical of wise people. I

³ Decade of 1920s, when there were still some important discoveries in physics. Nonetheless, I think Spengler is right in his prediction about the decline of the scientific world, as well as the decline of culture in general, although it was not as soon as he predicted. In my opinion, he was beyond his time, and he saw the problems of the future in our civilization in a prophetic way.

know well your little secret: will to power. In regard to this topic, Nietzsche has made a deep psychological analysis of men's intentions:

"Why do we try to demonstrate the truth? Because of a larger feeling of power, because of the utility, because it is indispensable. Summing up, in order to get some advantages. But this is a bias, a signal which points that deep down we do not worry about the truth." (Nietzsche, "The Will to Power")

The fight among specialists from different branches is similar to that for defending the lands in the medieval age. The "authorities of the matter", as they call themselves, are like lords of some lands who guard fervently their kingdom. When an intruder tries to insert his nose in a specialty which is not his, he will soon receive a cohort of "authorities" reading his rights. Generally, the lands are also fenced with a language and symbols to be crossed only by experts. In some occasions, I would say that formalisms are made to frighten other people, in order to make the entrance difficult.

Saying that doing research is collaborating for the peace and fecundity of mankind's progress⁴ is slightly naive. Nations do not invest on research today because of beautiful phrases like the above one. Nations, like persons, look for prestige. A country sends its sportsmen to the Olympic Games to win prestige, in order to get people to say: 'sportsmen with certain nationality won a medal...', and then the national hymn will be played and all that. Next day, the newspapers publish in their pages "our sportsmen won some medals in ...", this "our" makes the reader feel proud to belong to his country and then he will like to produce for his society. In the same way, the State pays scientists, even non-technological ones. If they are not useful for industrial production, they are at least useful to produce prestige. It is very beautiful to find in the news: "a scientist of our research centres discover...", it makes the citizens believe he lives in a true country. There are meetings about science even in undeveloped countries, do they also want to collaborate for peace and the fecundity of mankind's progress while their citizens live on poverty?

The great Spanish philosopher Miguel de Unamuno, expressed in the presence of these events: "let them invent!". This expression has contained more than a simple rebuff. If we are interested in knowing the truth, the way is not through microspecialization. Let the nations invest their efforts, their own pride will announce the news to the world and the ideas you were interested will arrive at your ears. Of course, this position does not include neither a job nor a medal, only wisdom and prudence.

"Do they invent things? Invent them! Electric light is here as good as in the place where it was invented. (...) On one hand, science with its applications is useful to make life easier. On the other hand, it is useful to open a new door for the wisdom. And are not there other doors? Have not we another one?" (Unamuno, letter to Ortega y Gasset)

"Yes, yes, I see it; a huge social activity, a powerful civilization, a lot of science, a lot of art, a lot of industry, a lot of morality, and then, when we have filled the world with industrial wonders, with large factories, with paths, with museums, with libraries, we will fall down exhausted near all this, and it will be, to whom?, was man made for science or science made for man?" (Unamuno, "Tragic Sense of Life in Men and in Peoples")

⁴ This was said by the king of Spain, Juan Carlos I.

That is, to whom? Perhaps, the historical moment when we must raise again the question has already arrived. Where are we going? The scientific method is awfully eroded. That thing with a reason for being at the beginning of the modern age as a promoter of positive knowledge; that later century of enlightenment;... all that is part of the past. Today, science is as crushed as the contemporary art. In words by Feyerabend in his “Against method”:

“Science failed to be a variable human tool to explore and change the world and rendered itself into a solid block of knowledge, impermeable to human dreams, wishes and hopes.”

Science loses its first attractiveness, simple technical operations remain. Which is the thing in whose name we do research? In the name of truth? Of economy? Of prestige? Max Weber thinks that the dreams of a science as a way to the truth, or happiness, or knowledge of God, etc. are shipwrecked. Neither the scientist is a prophet—says Weber. Science as an amusement still remains but the growing pedantry and smugness limits it.

Doing research is fighting, what else human beings could do? To fight against the power of others or to attain our own power, that depends on us. Science can be a revolution or deadlocked idleness. Still waters, without hitting the stones along their history, tend to form bogs.

Where is the Science?

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Science is used and misused today in a great variety of ways, each one of them is of utmost relevance for human life and activity. The policy of world leaders found it necessary for science to be applied for military needs and developed nations generously fund science for military purposes.

New, sophisticated, powerful weaponries are produced this day as a result of scientific achievements. It was also found beneficial to have science work towards the creation of a more comfortable life. Highly-developed technologies, industry, manufacture, farming, agriculture, commerce, services, transport, communications are science-based this day. Education, culture, civilization, highly-qualified work forces are being generated this day on the basis of science. Everything that matters for most humans, namely wealth, fame and pleasures can be achieved, to a larger and larger scale today, by means of science.

Science is viewed today as an immensely beneficial resource to be exploited to the greatest profit. In this respect, everybody speaks today only of “technological transfer”, “competitiveness”, “innovation”, “leadership” and “intellectual leadership” through science. Science is everywhere in our epoch “oriented”: oriented toward the military machine, warfare, technology, industry, economy, education, etc. Today there is no longer the idea of simply science for the sake of science. It is everywhere determined and goal-oriented.

The scientists should feel good and flattered by such great interest that is being shown today by society on their art and trade. The fact is that science provided much to society for a long time: mechanical constructions, thermal machines, electricity, nuclear energy, materials, electronics, and it is natural for society to try to control, accelerate and harness all this process of profiting by using and abusing science.

Nobody is satisfied yet with such a policy, all around the world. The taxpayers demand more and more from science and scientists feel more and more incapable of responding to such high demands. The reason for such failure resides in the inadequacy of this scientific policy.

Indeed, science is not funded accordingly to this policy unless it does generate something relevant to society, i.e. something which is useful for the military, industry, economy, education, etc. Scientific research (which is how science advances) is only meant today for the applications of science. Despite all these outlets of science, we find only its applications in various areas of activity and interest, but it is not science for the sake of science. It is science only for the sake of its applications.

The scientific policy today confuses science for its applications. By placing emphasis exclusively on the scientific applications we will end by having no true science anymore. Science is a resource like any others

and yet it is a bit special. Of course, scientific knowledge is not lowered nor degraded by repeated uses of it; it is not wasted nor dissipated. Newton's law does not vanish by being repeatedly used.

But people who have scientific knowledge and/or are concerned with the endeavours of maintaining at least, if not advancing, science; i.e. what we call scientists, will disappear, if not properly cultivated.

We have many applications of science, a serious endeavour for technological transfer, great expectations from using this science, but where is the science? We have not science as such anymore, but a policy that is aimed exclusively for its scientific applications, irrespective of how desirable and beneficial they are.

A very deeply-rooted mistake is to think that all scientists are in universities. This is profoundly wrong. In universities we have mainly professors who teach science to young people. We cannot say reasonably that teachers in universities do at the same time both science and teaching, because they do either, half of each, or half of none. It is more appropriate to emphasize the exclusive educational task of the universities and to provide separately for the scientists in distinct laboratories, institutes, etc. The great advances in science (scientific applications) were made by the former Soviet Union and the USA in the last half of the past century, precisely because these countries cultivated, separately, science from the scientists, and did not mix up science with its teaching nor its production.

Of course, these things are related, and it is desirable and profitable to cultivate such natural beneficial relations. How are we going to strengthen the relations between universities, scientists and high-tech entrepreneurs? Simply, by doing precisely what we want: providing for close relationships among such people, encouraging their meetings, discussions, talks, cooperation, etc. The main cause of the difficulties and insatisfaction today with the “failure” of science in society is precisely due to the vanishing relationship among scientists, “applicationists”, entrepreneurs and teachers. We need indeed to provide urgently for such close interaction but we have to be very careful not to mix up matters: to keep the distinction between these socio-professional categories. It is a scientific fact that distinctiveness and variety bring about force and motion, while the admixture increases only the ineffectiveness potential, and bring only a restful state.

If we are going to cultivate (by our policies) the distinction between scientists, teachers, professors, “applicationists” and entrepreneurs; to provide for close collaborative relationships among all them, while maintaining at the same time their distinction, in order not to mistake science for scientific research, teaching nor production, then we will be more scientific in our endeavours and more fortunate in our expectations.

We are yet pretty unscientific with respect to the basic issues. For instance, today we set for science the mission of reducing, or circumventing, the degradation of the environment, without noticing that every human activity degrades the environment. Indeed, even the mental processes degrade their environment, the brains in this case. Life is an organized process whereby the local entropy is diminished but at the same time we increase also the environmental entropy, just by living, so that the overall increase is higher than the decrease, and the process tends to equilibrium.

Of course, we will end with a more equilibrated world, where life is going to be extinct, because the fluctuations diminish near equilibrium. We would think then of finding a solution for preserving life by creating artificially another similar fluctuation, at a great expense in energy. The inherent limitations of such

an artificial process will then pose serious issues regarding how, who and how many are going to live that artificial life. This may be a serious problem for science and technology.

Another one is the process of thinking, for many believe that we should think about the thinking process in order to understand it. Firstly, they assume erroneously that there exists a conscience, or a consciousness, i.e. a state or process of thinking about the thinking, which is false. Anyone who thinks, and whenever anyone does it, is not conscious of what he or she is doing, there is no double thinking, the consciousness is identical with the thinking itself. Thinking is a natural process, associated with the complexity of the human brains, and does not “think of thinking”, because it is impossible, just do it. To think is just to be. Such sort of things we only learn through science, therefore, by providing in our policies for the proper cultivation of science, we will enhance by far our chances of responding to truly relevant questions.

Ethics in Science

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Note: Lectures on “Current Topics in Analytical Chemistry: Critical Analysis of the Literature”, 15 & 17 March 1994.

Is “ethics” a current topic in analytical chemistry?

Yes indeed!

Is there a literature on it to analyze critically?

Yes indeed!

How do I come to be interested in it? After about 25 years teaching chemistry and doing research in electro-analytical chemistry, I got interested in more philosophical questions, like:

Why do scientists study some things but not others? Why is it scientific to speculate about how the universe began but not scientific to study UFOs or whether the loch Ness monsters exist? What makes science so much more reliable than sociology?

So more and more over the last 15 to 20 years I've spent my time in what's called “science studies”, or “science & technology studies”, which tries to understand not only how science works but also how it affects society and politics and religion and how those affect science. Nowadays, a lot of interaction between science and the rest of society has to do with ethical questions about science. For example, in *C&E News* of 14 February [1]:

The scientific community must face the issue of scientific misconduct head on. It must work actively to prevent misconduct and not brush it under the rug when it occurs.

These actions are urged by . . . the National Academy of Sciences . . . , the National Academy of Engineering, and the Institute of Medicine

What brought that on? Such cases as this [2]:

A Michigan judge ordered the University of Michigan . . . to pay \$1.2 million in damages to a scientist after a jury found that her supervisor had stolen credit for her research and that the university had failed to investigate properly.

If you read *C&E News*, it seems almost as though there's an epidemic of wrong-doing":

Last July [3]: “**Misconduct cases include two chemists:** Leo A. Paquette, professor of chemistry at Ohio State University; and James H. Freisheim, former chairman of the Department of Biochemistry and Molecular Biology at the Medical College of Ohio . . . plagiarized grant applications the scientists had reviewed”

Last August [4]: “Kekulé was a German supernationalist who invented the dream [about the ring structure of benzene, a snake biting its tail] so he wouldn't have to cite previous work . . . by researchers from Austria, France, and Scotland”

Last November [5]: “**researchers often encounter scientific misconduct**—faculty and graduate students in four disciplines—including chemistry . . . have encountered scientific misconduct and a variety of dubious research practices”

Last year Professor Harry Gibson gave colleagues in the Chemistry Department copies of his letters [6] to a university and a granting agency about a proposal he had been sent to review. He wrote, “Unfortunately, the proposal was plagiarized from my proposal of 1990”.

Some years ago I had a letter from a friend in Australia who had discovered that one of his post-docs had been leaking results and research materials to a competitor overseas.

In his memoir *The Double Helix*, Nobel-Prize-winner J. D. Watson described getting data that its owner would not have wanted him to see.

William Lipscomb, 1976 Nobel-Prize-winner in chemistry, says that he “no longer put my most original ideas in my research proposals, which are read by many referees and officials. I hold back anything that another investigator might hop on and carry out. When I was starting out, people respected each other's research more than they do today, and there was less stealing of ideas” [7]

Rustum Roy, Professor of Materials Science at Penn State, himself an outspoken critic of some corrupt practices in modern science, used a press conference to announce a new method for making synthetic diamond, and justified that as “the only way to prevent . . . a small group of peer reviewers . . . [having] an advance chance to duplicate the work in their labs” [8]

In X-ray crystallography, it had become routine to publish structures of complex substances without giving the raw data, so that others couldn't do proper checks or build on the work [9]

In the hurry to develop high-temperature superconductors [10] “scientific results were announced first in the press to gain a few days on other groups. . . . [One researcher] applied for a patent [and then] submitted a paper containing two systematic mistakes making it useless to any reader. . . . [and gave] a press conference . . . announcing-without giving any detail-the discovery Only . . . at the latest possible date, did he send his corrections to the journal”.

I hope you agree that all this is unpleasant, sleazy, and shouldn't happen. But does it have anything to do with the actual science? Does it really matter, who gets the credit, so long as science keeps progressing?

I think it *does* matter—because science progresses with sound, reliable results *only to the degree that scientists are honest*.

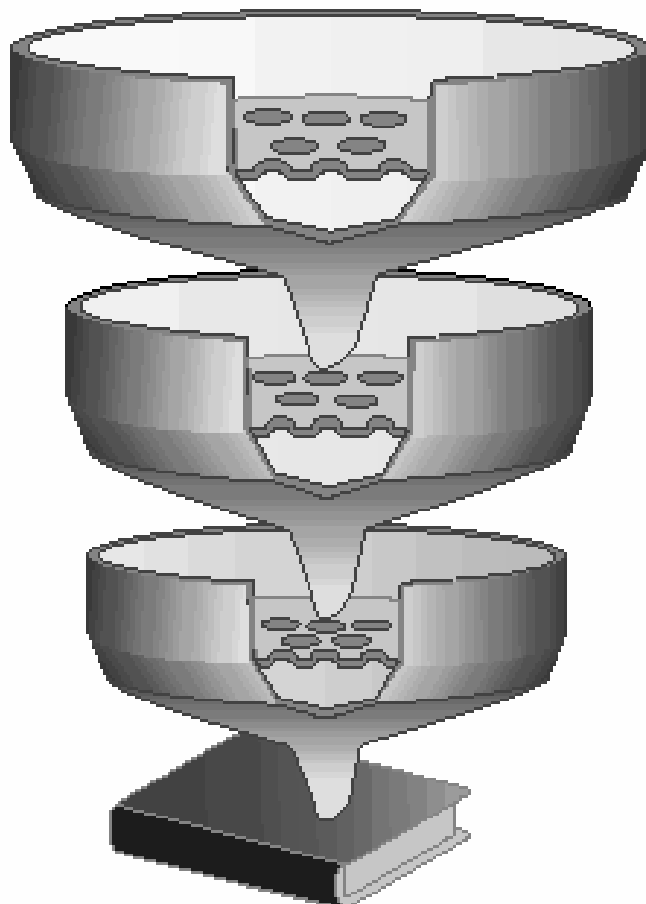
Most people think science gives trustworthy results because of “the scientific method”: testing ideas by experiment and so either proving or disproving them. Isn't that what you all do? Experiment, and find out what's true and what isn't?

But what if an experiment doesn't give the result you expected? What if it gives a result that you just *know* is wrong in some way? Don't you keep trying until you get the “right” result? Especially if you know that your boss is very sure that's what you should get? Isn't there the temptation to fudge a bit? Since you know what the right answer *ought* to be, why not just round the numbers off a bit?

What would happen to science if *most* scientists rounded off and fudged? What would happen if they thought less about what the experiment actually shows and more about who wants which results, and what would be better for getting the next job or prize?

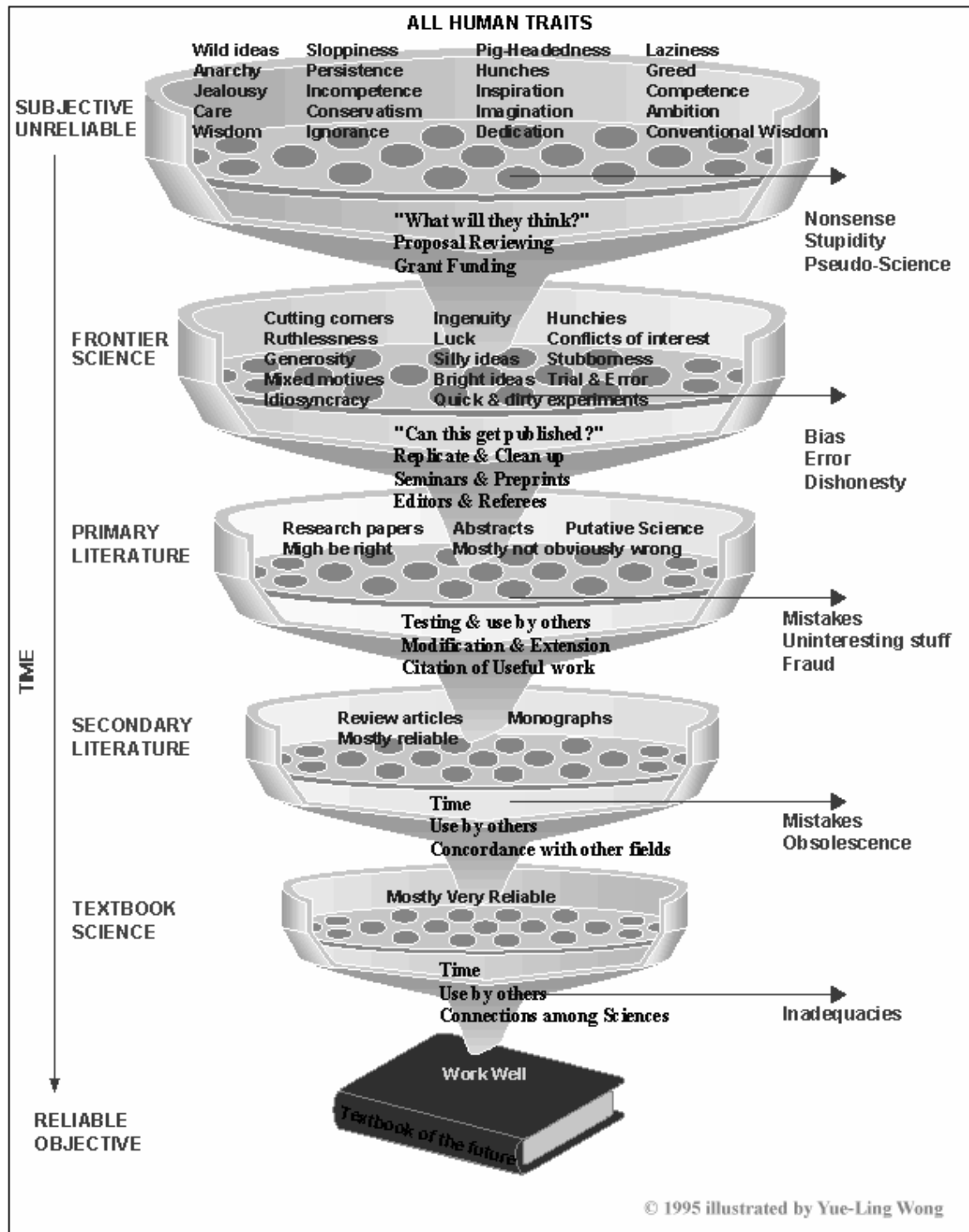
To understand why science may be reliable or unreliable, you have to recognize that science is done by human beings, and that how they interact with one another is absolutely crucial. Here's how I think scientific knowledge accumulates; I call it “the knowledge filter”:

Science gets done through a communal process that's like the action of a large filter funnel with several stages, through which the murky mess of humans making many, often contradictory claims of truths about the world eventually yields a little trickle of fairly clear understanding (Figure 1).



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The human urge to know and to convince others is a pandemonium of fantasies and folklore, hallucinations and religious cults, myth and pseudo-science, not just empirical and logical investigation. Just think of all the New-Age magazines and pop-religious paperbacks that flood the bookstores: dianetics and Scientology, extrasensory perception and reincarnation and channeling, astrology and Tarot, revelations from Nicholas Tesla and other neglected supposed geniuses. Nowadays, anyone who seriously wants to contribute to scientific knowledge had best not start there but rather get some undergraduate and graduate training in science: that narrows the mouth of the filter funnel, by educating about already established knowledge, about what's plausible and what isn't (Figure 2).



Learning to do research means learning to ask yourself all the time, “What will others think about this?”, because those others will examine you and decide whether you'll graduate or not, whether you'll be recommended for jobs and grants—or not, whether your papers will get published—or not, whether you get promotions and better jobs and prizes—or not.

Through that awareness that you have to satisfy the opinions of others, and through the actual practice of having colleagues and competitors look over grant proposals and manuscripts, much nonsense, pseudo-science, and stupidity is still-born, or at least filtered out before it's gotten very far.

So the ferment of scientific research, of *frontier science*, is a bit more disciplined than the general intellectual level of society as judged by what's on bookstore shelves. But research is still a rather messy business. Scientists differ in competence and in integrity; they're rebellious in varying degrees, toward established knowledge and toward established practices; they vary in creativity, interests, judgment, patience, and so forth.

But what they produce becomes a little less subjective, a little less imperfect by the time it gets published, because in order to get into print, it isn't enough to be personally convinced of some scientific fact—not even if you've become convinced by observing, setting up hypotheses, and testing them: you have to show evidence strong enough to persuade others that you're right, or at least that you're not obviously wrong. You have to produce evidence strong enough to convince people who start with different beliefs and prejudices than you do. So the *primary literature* of articles in research journals is more disciplined, more objective and less personal than what goes on in individual labs.

Still, the primary literature is anything but entirely consensual—there are competing theories and even competing, apparently contradictory results. As John Ziman has pointed out, the primary literature isn't scientific knowledge, it's merely information that certain claims have been made [11]. If those seem interesting enough to others, they'll be used and thereby tested and perhaps modified or extended—or found to be untrue. Whatever survives as useful knowledge gets cited in other articles and eventually in review articles and monographs, the *secondary literature* which is considerably more consensual and reliable than the primary literature.

But still that's far from gospel. If after still more use and modification, including use by people in other specialties, if still no damaging flaws have turned up, then the knowledge is likely to get into textbooks. This *textbook science* is very reliable. It's been cleansed of most of the personal bias, error, and dishonesty that may have been there originally.

Yet even this textbook science isn't objectively true knowledge. Ziman guesses that while the primary literature in physics is perhaps 90% wrong, textbooks in physics are perhaps 90% right—by no means 100% right. The next century's textbooks of science will be significantly different from today's, even more than by the 10% that's wrong in today's, because there will not only be correction of errors but inclusion of things that we can't even dream of at present.

This knowledge filter illustrates that it's peer review, and the awareness of peer review, and the passage of time that makes scientific knowledge non-subjective and reliable. But there's nothing automatic about peer review or self-discipline. If peer review is cronyism—if scientists believe it proper to praise their friends and

relatives rather than meritorious work irrespective of who does it—then false views and unreliable results will be disseminated.

Contrast this filtering with the popular notion of an “information explosion” that implies a crisis of coping with new knowledge; when rather it's a matter of weeding out from a mass of rubbish, a small amount of valid, useful, meaningful stuff. This model would suggest a different way of doing things than is now the generally accepted one. We seem to think that more research is always better, and that publishing original research is more worthy than writing review articles or books. But perhaps, given the mass of rubbish that needs filtering, perhaps *less* research would be better than more?! Maybe writing review articles and textbooks should be rewarded more than producing research articles?!

There's another model of how science gets done that I think is useful. It was suggested by Michael Polanyi, a chemist who turned philosopher of science (and whose son John won the Nobel Prize in Chemistry not so long ago). Polanyi asked, how can people best work together to produce good science as efficiently as possible? And he thought of other cooperative activities. Let's say you have a wagon-load of potatoes to peel and lots of people to help: you give each of them a pile of potatoes and ask them to peel them. But science wouldn't progress efficiently if every scientist were trying to do the same thing at the same time; it works best when scientists specialize and also critique one another. In any given study, people get ideas at different times, they get different ideas, they have different experimental skills—how to arrange it so that whenever a particular idea or experiment is called for, the best qualified person knows that it's time to step in?

Only by ensuring that everyone knows what everyone else is doing, by having open and honest publication. Polanyi made the nice comparison that the sort of cooperation you want in doing science is the same as when a group of people are jointly working a jigsaw puzzle: everyone does what they can do best, everyone works at their own pace, and since everyone knows what everyone else is doing, and what the present state of the puzzle is, everyone is able to step in at the right place at the right time.

So my first main point is that to understand science, its history and development, its contemporary state, what it can do in the future, what role it can and does play in society—you need to think of it *not* as individuals practicing “the scientific method” but as people working at a jigsaw puzzle and filtering knowledge. There are lots of things about science that you can understand through the puzzle-&-filter model that you can't understand in terms of a “scientific method”:

	Scientific Methods	Jigsaw & Filter
17th century Scientific Revolution	?	Societies, journals - community formed
Greece, China, etc	??	as above
Textbooks reliable, new research fallibl (90% right vs. 90% wrong)	??	filtering takes time
revolutions in science: oxygen vs. phlogiston; particle-waves; making previously "impossible" molecules	method reliable and yet leads to perpetual self-correction??	new connections in jigsaw: it isn't the sky, it's water! or, those pieces don't really fit together!
testing ideas: cold fusion; N-rays	scientists are able to be objective even when it harms their career; can think of crucial tests of pet ideas	keep one another honest; love to prove others wrong
experimentalists & theorists; idea people and kibitzers; geologists and physicists re extinctions	??every scientist uses the same scientific method??	human diversity: side-piece specialists; color vs. shape; systematic vs. inspirational; speculating vs. playing around
expertise, greatness: very, very few scientists have more than one breakthrough to their credit	those who are most expert at using The method will do most of the great work	every discovery results from a unique combination of inspiration, persistence, luck
where do ideas come from?	??	
How can you detect pseudo-science? Why is astrology pseudo-science? Or ufology? Why was acupuncture pseudo-science a few decades ago but not now Or ball lightning? Giant squid?	Pseudo-scientists don't use The Method? Or misuse it? What about Harold Edgerton at Loch Ness? Sonar, strobe photography, statistical analysis, trial and error... What basis not to publish an article if the methods seem sound??	need a large and diverse enough community to keep one another on the right track; pseudo-scientists are hermits. There are degrees of isolation: Soviet genetics; N-rays; Wilhelm Reich
More research	Always good	More rubbish to filter
Faster research	Good!	Need time to filter the rubbish out

So why has the myth persisted, that science is what it is because of the scientific method? There are a number of reasons:

1. Practicing scientists don't much care about explaining why science works, they're interested in doing it. They're happy to let the philosophers and sociologists worry about philosophy and sociology.
2. Philosophers and sociologists don't pay much attention to scientists; and they don't know much about the actual practice of science. What they understand is abstract methodology, and theory, and theory about method.
3. The only claim that sociology, political science, and psychology have to being scientific is that they use the scientific method. Those fields haven't accumulated reliably applicable, predictably useful knowledge.
4. Some people argued that if science *doesn't* use the scientific method, if it's "just" a human activity, then there's nothing especially rational or reliable about it.
5. And we don't like to give up the possibility of being quite certain about things. About a century ago, we discarded religion as the revelation of truth because science seemed to explain a lot of things better; if we now give up the certainty of science, what's left?—at least for Marxists or secular humanists?

At any rate, it's still a firmly entrenched belief that science works by the scientific method; which is objective, self-correcting, *impersonal*. That's why, when the public fuss about misconduct in science really got going 5 or 10 years ago, most scientists dismissed it as making mountains out of mole-hills. So what if a few silly people think they can cheat in science and get away with it? We know that they can't.

* * * * *

The knowledge filter works properly if and only if peer review, mutual criticism, is objective, impartial. You need to judge work by how good the work is, not by who did it or where they're from. The jigsaw puzzle gets put together efficiently and properly only if the players are open with one another and behave as honestly as they can.

Nowadays, many people don't seem to understand that honesty is the best policy in science; for instance the AIDS activist who said, "Show me someone with a perfectly clean record who's able to get anything done" [12].

There have been literally dozens of books published in the last ten years or so telling the supposedly true inside stories of successful scientific discovery as episodes of cut-throat competition and cutting corners by scientists anxious to get there first and win the biggest prizes and grants:

Year	References
1968	James D. Watson, <i>The Double Helix: A Personal Account of the Discovery of the Structure of DNA</i>
1981	Tracy Kidder, <i>The Soul of a New Machine</i> Nicholas Wade, <i>The Nobel Duel: Two Scientists' 21-Year Race to Win the World's Most Coveted Research Prize</i>
1985	David H. Clark, <i>The Quest for SS433</i>
1986	Robert Kanigel, <i>Apprentice to Genius: The Making of a Scientific Dynasty</i> David M. Raup, <i>The Nemesis Affair: A Story of the Death of Dinosaurs and the Ways of Science</i> David Taubes, <i>Nobel Dreams: Power, Deceit, and the Ultimate Experiment</i>
1987	Stephen S. Hall, <i>Invisible Frontiers: The Race to Synthesize a Human Gene</i> Roger Lewin, <i>Bones of Contention: Controversies in the Search for Human Origins</i> Ed Ergis, <i>Who Got Einstein's Office: Eccentricity and Genius at the the Institute for Advanced Study</i>
1988	Natalie Angier, <i>Natural Obsessions: The Search for the Oncogene</i> Sheldon Glashow (with Ben Bova), <i>Interactions: A Journey Through the Mind of a Particle Physicist and the Matter of This World</i> Jeff Goldberg, <i>Anatomy of a Scientific Discovery</i> Robert M. Hazen, <i>The Breakthrough: The Race for the Superconductor</i> Charles E. Levinthal, <i>Messengers of Paradise: Opiates and the Brian</i> Bruce Schechter, <i>The Path of No Resistance: The Story of the Revolution in Superconductivity</i>
1989	Solomon H. Snyder, <i>Brainstorming: The Science and Politics of Opiate Research</i> Robert Teitelman, <i>Gene Dreams: Wall Street, Academia, and the Rise of Biotechnology</i>

I said just now, puzzle players need to “behave as honestly as they can”; because *complete* honesty and objectivity aren't achievable by the typical human being. It's difficult to award grades without being influenced by how you like or dislike a student's manner. It gets especially difficult if the student is also a friend, child, spouse, or lover. That's why having such people in your class should be avoided: it protects you from an intolerable conflict of interest, between wanting to be fair and wanting to please someone you care about. It also protects the rest of the class from being suspicious of the teacher and jealous of his protégé. Conflicts of interest erode the fabric of trust on which worthwhile social interactions rely.

People don't seem to understand that any more. Influential voices are suggesting that we ignore conflicts of interest in order to get important things done: new drugs developed, better facilities for universities established, the balance of trade improved. I think this rush to get things done even if it means doing risky things, cutting corners, is a real threat to science. I think the effect can already be seen in the “hottest” research areas of molecular biology and genetic engineering.

In September 1989, the National Institutes of Health (NIH) proposed that people applying for grants should disclose all sources of support, including honoraria and consulting fees; and that people funded by NIH (or their assistants, consultants, spouses, or children) shouldn't own stock in companies that would be affected by the outcome of the research; and that results could not be shared with private firms before they had been made public.

Doesn't all that sound reasonable? Yet NIH was flooded by protests [13]:

The Association of Biotechnology Companies said the guidelines were a “draconian remedy which . . . will create real problems for the biotechnology industry”. What problems?

The guidelines would “effectively eliminate contact between academia and industry for many small biotechnology companies” that don't have the financial resources to pay researchers in cash for their work. Well, so what? Why should companies, large or small, be able to get something without paying for it? How many companies in other fields than biotechnology are able to get going without the necessary financial resources?

One private Cancer Research Center complained [14] that keeping all those financial records would take 4.3 feet of file space per year. Does that sound like a reason or like an excuse?

Some people asked, how could investigators know beforehand which companies might benefit from the fundamental research the investigators did? I suppose the best answer is, because they're not stupid. But in any case, if unexpected applications of some work open up, you can explain what happened and find some legitimate way of getting rid of the stock. Again, this is an excuse rather than a reason.

The NIH guidelines would have prohibited investigators from taking money from companies whose products they were evaluating in a government-funded project. Seems a sensible enough safeguard, doesn't it? When the Air Force tests a new airplane, we do hope the testers aren't getting something from the aircraft company.

Or do we? “Clinical researchers . . . pointed out that ties with industry were crucial for rapid progress”. But who was arguing against ties with industry? Only against those ties *that produce conflicts of interest*. And what is this preoccupation with rapidity? Is it better to be fast than to be sound? Is it better to get false

results quickly than valid results slowly? Doesn't the knowledge filter demand time as well as disinterested participation?

Those anonymous clinical researchers also argued that “an individual's bias . . . could hardly be a major factor in influencing the outcome of rigorously controlled multi-center trials”. But where's the rigorous control if conflict of interest is permitted? And if it's permitted, why assume that only one person would suffer from it rather than everyone, in all those multiple centers?

George Levy directs a lab at Syracuse University; he also owns a company that exploits commercially what he does in his university lab, and the company pays royalties that support his lab. “Undoubtedly, I have split loyalties. That really is a problem”, he said. “But the alternative is to let the Japanese buy the United States” [15]. Really? Don't they have rules against conflicts of interest in Japan? Is it really for that reason that the United States has lost out in trade to Japan, or is it for such reasons as inferior quality control and inept management?

Levy also said [16], “If you take away anybody with a conflict of interest, you take away all the experts”. Mark Wrigton, chairman of the Chemistry Department at MIT, was “very concerned about the development of guidelines that will have the effect of precluding the opportunity to pursue the very research that is most interesting”. But how do those guidelines preclude any work from getting done? And interesting for what reasons and to whom? Interesting for scientific reasons or because there's money to be made out of it?

“Blanket prohibitions don't work”, said the Vice-President for Research at a major university. I suppose he believes that all existing laws should be wiped from the books, like against drunken driving.

These protests against the NIH guidelines showed that influential people don't understand the dangers posed by conflicts of interest; or, that they believe that ends justify means; or both. A spokesman for the National Association of State Universities and Land-Grant Colleges said, “Many people have apparent conflicts of interest”. But what's an *apparent* conflict of interest? How does it differ from a *real* conflict of interest?

Perhaps that spokesman was thinking—or rather *not* thinking—along the same lines as Nobel-Prize-winner David Baltimore when he was a professor at MIT and about to become in addition, at the same time, Director of the Whitehead Institute for Biomedical Research. How, he was asked, would he handle that conflict of interest? Well, Baltimore replied [17], “I think people are entitled to ask that of me. But I do think the statements and decisions I make come from the highest sense of integrity”. The conflict is only apparent, in other words, no harm will come of it.

Baltimore's blind spot owes something, I think, to belief in the mythical scientific method. Baltimore is the most successful kind of scientist, having won the Nobel Prize; scientists, so goes the old syllogism, use the scientific method, which guarantees objectivity; therefore, Baltimore, having shown that he's a master of that method, must be particularly able to be objective in all things at all times.

But if we understand that science is a human, social activity, we may be less likely to swallow that fallacy. Among the things we know about human beings is that they—meaning we, all of us—don't see the world directly, we “see” *an interpretation* of what our eyes and their nerves signal to the brain. It's the same with intellectual signals or information: we “see” the data through the distortions of our beliefs and

preconceptions. We tend to see what we expect to see and want to see and we're very apt to overlook things that we don't expect or want to see. That's all largely *automatic*, we don't have much control over it. Even when we're aware of the *general* problem, it's very difficult to counteract and it's impossible to counteract perfectly and completely in every actual instance.

Financial advisers and salesmen, not to mention outright confidence tricksters, make good livings because they understand how little encouragement most of us need to fool ourselves into imagining something to be true just because we want it to be true. There's a book about this that I highly recommend, by Thomas Gilovich, called *How We Know What Isn't So* [18].

The plain fact of the matter is that no human being can be trusted as much under a conflict of interest as when there's no such conflict. It isn't that people will always do the wrong thing; it isn't that we *always* put our private interests ahead of the public interest; it's just that if you have several, mutually conflicting wishes or interests, then you can't satisfy all of them. So it's to the public's interest—which means to everyone's interest—that those who serve us shouldn't have reasons for wanting to short-change us.

And still, influential people don't understand that. A television program (NOVA) a few years ago showed many doctors, clinicians, researchers who thought it quite all right to give speeches praising the value of a given drug even when the manufacturer of the drug was paying them to give the speech. The chairwoman of a clinical department said that her opinion could be entirely objective even though her department was getting a lot of money from drug manufacturers.

A spokesman for the American Association of Universities—the most prestigious organization of research universities in the United States—said that since everyone has conflicts of interest we shouldn't try to avoid that in government-funded research, we should just decide how much of it to allow. But you can't quantify ethics or conflicts of interest like that. Quite the contrary, our experience of life and of history is that small breaches of honesty tend to lead to bigger ones.

Another misconception about conflict of interest is that it's a personal matter, a problem for individuals. But institutions too can and do suffer from conflicts of interest. Universities, for example, need money to do the desirable and valuable things they do: but is it permissible for a university to do everything and anything to get money for those worthwhile goals?

Accepting gifts from parents, graduates, and other benefactors has long been standard practice, and it doesn't usually cause trouble—unless, of course, a wealthy donor has a stupid nephew whom he wants enrolled and given a degree.

Is it quite all right for universities to operate book-stores, cafeterias, stores that sell mementos?

What about cashing in through patents on discoveries made by the faculty?

What about setting up a for-profit company to exploit such discoveries, instead of just getting royalties from private companies that buy patent rights? That's what the University of California planned to do last year, before protests called a halt to it.

What about hiring lobbyists to persuade the government to designate some funds for a new building or program? That's become a growth industry. Universities hire—for 6-figure fees—people who try to persuade members of Congress to put into some bill, say \$60 million dollars for a supercomputing center at Cornell

University. That's an actual example from about ten years ago. On that occasion, Cornell's President said that his university wouldn't accept the money: they were perfectly able to compete through the traditional, peer-reviewed channels that decide which university can make the best use of funds for any given project. But a vice-president here at Tech sneered at the scruple of Cornell's President, saying it was a fine example of allowing principles to get in the way of getting things done.

This new method of using political clout rather than intellectual merit to make decisions, pork-barreling in other words, is an almost hallowed American tradition but it has only recently been taken up by universities. The American Association of Universities admitted that this pork-barreling is a bad thing, and wished that it wouldn't happen; *but it refused to criticize those of its members who were doing it*, on the grounds that *the need for resources is so great*.

In other words the Association was saying that the ends justify the means. But ends can *never* justify means, because it's the means you use that determine the ends that you'll get. Use force instead of persuasion, and you'll have a society that's controlled by force: use pork-barreling to get what you want, and you'll have a society that works through bribery and who you know and not on the basis of merit.

So conflicts of interest exist at all levels, institutional as well as personal, and we're in danger of forgetting how deleterious they are. One way of trying to avoid the issue is to pretend that it doesn't exist. The Pharmaceutical Manufacturers Association, for example, assured us [19] that "In most instances, private interest and public interest can coincide without interfering with the objective conduct of publicly funded research". If any of you are as old as I am, you might recall from forty years ago when Charles Wilson, who had been President of General Motors, was nominated to become Secretary of Defense. How would he handle the conflicts of interest, he was asked, since General Motors is a large contractor to the Defense Department? No problem, replied Wilson, "What's good for the country is good for General Motors, and what's good for General Motors is good for the country". Back then, Wilson was publicly ridiculed and featured in cartoons and comic-strips; nowadays, there's no public outcry when people say things like that.

Yet the truth about conflict of interest remains the same. No human being with conflicting loyalties or interests can always be relied on to act in the public interest, and so we do well to remove as much temptation as possible from as many people as possible. Perhaps then the pace of innovation might slow down a bit, and some people might not be able to make a lot of money in a short time; but perhaps we could also avoid some of the disgraces that have been in the news over the last decade or two, like the clinical researchers who on the side owned a company making eye ointment and who suppressed their findings that the ointment is ineffective, until they had sold the stock in their company.

Let me leave you with the thought that these issues are *not* other people's problems, they are *our* problem, they are *everyone's* problem. Here are a few typical ethical problems we all face:

How much time to spend preparing for teaching, or in grading lab papers or exams, when we need the same time to do our research and prepare for our own exams?

How many people outside the immediate research area should be on a student's committee, to make sure that the student isn't just exploited as a "pair of hands" on a big project?

Similarly, who should have a say in drawing up and evaluating cumulative or comprehensive exams?

Should faculty with large grants be able to offer special inducements for students to work with them rather than with professors who don't have large grants? Shouldn't students be free to choose their own dissertation topics?

Faculty evaluating others for tenure or promotion; administrators deciding how to calculate overhead charges, and how to distribute money collected as overhead; program managers in funding agencies; scientists reviewing research proposals and manuscripts intended for publication—in all those situations and many others, we as individuals have to decide how much weight to give to sheer merit as opposed to other considerations like scratching one another's back. If most of us choose the ethical thing, then science will continue to prosper. If too many of us cut corners, then science can come to a dead stop.

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Is Ethics in Science an Oxymoron?¹

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The planet is in peril, our world is experiencing serious problems as exponential population growth, environmental devastation, climatic change, an impending flu pandemic, energy shortages, widespread violence, hunger and poverty, unbridled capitalism, globalization of selfishness and corruption, clash of civilizations, religious fundamentalism, extinction of animal species, disappearance of cultures and languages, nuclear proliferation, threats of biological and cyber-terrorism, to name a few. For these reasons we cannot afford to suppress the work of innovative minds whose novel ideas could revolutionize the world for the better, like discovering a new clean energy source, a cure for aids, cancer, genetically-induced illnesses... etc. In short, the human race cannot bear the repression and censorship of novel ideas that might pose challenges to the present paradigms of knowledge; ideas that might save the planet one day such that the select-few would not be forced to abandon our Earth-mother-ship which has been sailing through space during the past four and half billion years. Life saving ideas so it will not be necessary to replace polar bears for solar bears...

To illustrate how serious is the lack of ethics in physics and science, I will go back to Harvard University during the 1970's when S. Coleman, S. Glashow, S. Weinberg and other senior scientists destroyed the career of Ruggero Maria Santilli. They will argue to the contrary, that it was Santilli himself who destroyed his own career. Incident that it was mainly due to their adamant opposition to non-Einsteinian generalizations of Galilei's relativity. His full account can be found in Santilli's book "Il Grande Grido: Ethical Probe on Einstein's followers in the USA" (Alpha Publishing, 1984). "Il Grande Grido" is the Italian expression for "The Great Outcry".

Nowadays we see almost every day papers posted in the internet related to Lorentz violations, Quantum group deformations of Poincaré symmetry, twisted Poincaré invariance of deformed Noncommutative Quantum Field Theories, non-isotropic Relativity based on Finsler geometry,... etc. Worth mentioning is the fact that for decades the West ignored completely the work of the Romanian school on Lagrange-Finsler geometry, whereas today many have jumped onto the bandwagon claiming to be the true pioneers and without citing the crucial work by the Romanian scientists.

¹ This essay is dedicated to the loving memory of the Swedish activist Brigitte Naroskyin who died tragically in Antibes, France. During her short life she fought feverishly for justice everywhere.

The aforementioned Harvard high priests of science opposed Santilli to the point of preventing him from drawing a salary from his very own grant money for almost one academic year. The prohibition to draw his salary was perpetrated with the full awareness of the fact that it would have created severe economic hardship on Santilli's children and wife. He had at the time two children at a tender age and a wife to feed. In addition to this, according to Santilli's accounts in his book, the Harvard scientists tried to prevent Santilli from giving seminars in the Boston area and made sure that his name was deleted from the Boston area weekly calendar of talks. This is not an isolated incident. It occurs all the time, for example, the Austrian molecular biologist, Alfred Schoeller, experienced similar treatment at the University of Texas at Austin and later on at the Hospital in Vienna, when he was literally kicked out into the streets, while his professors confiscated his very own grant money. Alfred Schoeller took to court (thanks to private donations) those individuals involved and despite proving why they had lied in 19 occasions, the Austrian court ruled against Alfred Schoeller and he found himself penniless and without job prospects. If it wasn't for his family he may not have survived.

Matti Pitkannen has had similar experiences when the powers that be at the University of Helsinki, Finland, forced him out of academia. He and his family has survived on a shoe-string budget during the past two decades thanks to the welfare of the state. Against all odds, Pitkannen has managed to pursue his novel work on Topological Geometro-Dynamics.

At the end of Santilli's 354-page book, he wrote a series of recommendations to the US Congress, the American Physical Society, many directors of Federal Agencies,... concerning the need to establish a panel on scientific ethics. It all fell on deaf ears. Santilli even sent numerous copies of his book to newspaper editors, libraries, colleagues,... to no avail. For the most part, he was not even honored with an acknowledgment of having received his book. I am not necessarily saying that Santilli is the crowning achievement of human perfection. In recent years he accused me of being a jewish spy for Paul Ginsparg (who runs the arXiv.org e-prints at Cornell) and of gathering secret information about impending class action lawsuits against Ginsparg and Cornell University for the blacklisting of scientists. My real name is Perelman, my father was jewish who was born in Otaci, Bessarabia (Moldava). Speaking of Perelman, the story of Grisha Perelman has become famous today when he refused to go to Madrid and receive the Fields Medal in Mathematics (the Nobel prize analog) in the hands of King Juan Carlos of Spain for his solution of the Poincaré Conjecture. Grisha Perelman most likely, if he has not done so, will refuse the one-million-dollar Clay Mathematics Institute award for proving the Poincaré conjecture (one of the Millennium problems). The reasons are varied but according to a New Yorker article, it seems that Grisha Perelman experienced back-handed stabbing on the part of the Mathematics establishment led in most part by S. Yau at Harvard University. Grisha Perelman is now jobless and living with his mother in a small apartment in St. Petersburg on a pittance. His main focus now is to play the violin, to be left alone and to stay away from the corrosive and suffocating world of academia.

Other famous stories of injustices in science are those experienced by Stueckelberg who deserved to receive the Nobel Prize in 3 occasions but never got it. In particular for the creation of Quantum Electrodynamics. According to the accounts of Frank

(Tony) Smith (another blacklisted by the arXiv.org at Cornell University), Feynman himself acknowledges this fact in his talk at CERN (Geneve, Switzerland) when he said at the end of his lecture that while he basked in all the glory, Stueckelberg, the man who deserved all the due credit, walked alone into the sunset. Frank (Tony) Smith in his website: <http://www.valdostamuseum.org/hamsmith> also discusses how Oppenheimer made famous the quote: “If we cannot disprove David Bohm, we must choose to ignore him”, quote that spearheaded the silence treatment and ad hominem attacks on controversial scientists in the USA. Ironically, later on, Oppenheimer himself was attacked and fell from grace during the Senator McCarthy's witchhunt of Communists.

A particular example of the detrimental psychological effects of the “silence treatment” on a scientist is the story about Armand Wyler's sabbatical leave from the University of Zurich to the Institute for Advanced Study at Princeton University in the early 1970's. He was invited to Princeton by F. Dyson based on his interest on Wyler's work in bounded complex homogeneous domains and his geometrical derivation of the fine structure constant ($1/137$) involving the Electro-Magnetic interaction, and whose value is crucial for the emergence of life in our Universe. According to a personal communication with Frank Tony Smith (another blacklisted by the Cornell arXiv.org e-archives) the atmosphere under which Wyler had to live during his sabbatical leave at Princeton was filled with such hostile silence that Wyler felt utterly invisible. His “silence treatment” was so detrimental to Wyler's mental health that when he returned back to Zurich he basically experienced a nervous breakdown. Even worse, it seems that F. Dyson himself might have felt this hostile silence treatment towards Wyler to the point that he gave R. Gilmore (a Group theory expert) the only copy of Wyler's work during his Princeton stay. Wyler gave a copy of his recent work at the time (in June 1972) to Dyson. It was related to Operations of Symplectic and Spinor Groups and the Complex Light Cone. At the top of the page one can see Wyler's warm words (in French) thanking Professor Dyson. However, in the right hand margin one can read Dyson's words to R. Gilmore saying: “This seems to be the only copy I have. Don't tell Wyler that I gave it to you, F. D.”. Perhaps F. Dyson did not wish to hurt Wyler's feelings and this is why he asked R. Gilmore not to tell Wyler that he had dispensed of the only copy he had (containing the warm words of gratitude from Wyler to Dyson). Frank Tony Smith posted in his website the 57 pages of Wyler's two articles at Princeton. This was possible since R. Gilmore gave the alleged copy to Frank Tony Smith later on. One of the reasons why Frank Tony Smith has been blacklisted is because he extended Wyler's derivation of the fine structure constant to the weak and strong couplings (in addition to other parameters of the Standard Model). Ideas that have been ridiculed so many times by so many very powerful individuals (David Gross, for example). The consensus today is that we are forced to believe the String theory approach predicting 10^{1000} (or even an infinite number of) vacuum solutions to the possible Universes. They claim that anything else besides string theory is just rubbish and nonsense. Christian Beck in London has written a book (World Scientific) and many articles obtaining all of the fundamental parameters of the Standard Model based on Stochastic Quantization methods in Kaneko lattices, but his work has largely been ignored by the scientific community. This behaviour is indeed consistent with the above Oppenheimer's famous words against David Bohm: “If we cannot disprove David Bohm, we must choose to ignore him...”.

A more recent incident at the Institute for Advanced Study at Princeton was the internal power struggle among Piet Hut and some theorists. Piet Hut (Astrophysicist) was one of the youngest permanent members at the Institute and had to sue the Institute because they were trying to get rid of him for what appeared to be his Buddhist beliefs. It was all documented in the article in the New York Times. Around 1999 George Chapline (at Lawrence Livermore) told me in Stanford University that there were rumours going around that N. Seiberg wanted to get rid of Stephen Adler (a permanent member) at the Institute for Advanced Study because of Seiberg's dislike of Adler's book on Quaternionic Quantum Mechanics. It does not surprise me if these rumours were true. Very intelligent people are not necessarily very nice.

Most recently I learned from Arkady Kholodenko at Clemson University (South Carolina) that it was Bachelier (before Dirac and Feynman) who in the early 1900's developed the theory of path integrals as part of his Ph.D. thesis in France, and that it was Kramer who came up with Matrix Quantum Mechanics before Werner Heisenberg. This in no way diminishes the great achievements made by those science idols, but illustrates that there are probably many unsung heroes who never will receive the proper recognition for the original work, whereas others take the full credit. A more current example is the fate of the work by Merab Gogberashvili in Georgia (former Soviet Union). Based on the pioneering work of Rubakov and Shaposhnikov in the 1980's, he proposed the brane-world model (our universe as an extended object living in higher dimensions) to solve the hierarchy problem in Particle Physics many months before L. Randall and R. Sundrum did, but the later authors gathered all the glory and book deals after their paper appeared in Phys. Rev. Letters. All this taking place while Merab Gogberashvili receives no funding whatsoever from his Institute at Tbilisi and supports himself by doing business transactions, according to what Eduardo Guendelman (Ben Gurion University) told me at a Relativity meeting in Santa Barbara, California in 2006, meeting that Merab Gogberashvili also attended.

Even more famously unbeknownst to many are the quotes made by John F. Kennedy in his inauguration speech: *"Don't ask what your country can do for you, ask what you can do for your country."* These quotes originally belong to K. Gibran, the author of the book "The Prophet", K. Gibran wrote an article under the same title when he was living in the Boston area. Article that J. F. Kennedy read as Massachusetts senator and which later inspired him. It was unfortunate that Kennedy did not reveal the name of K. Gibran after having quoted him. I learned about this story when I was browsing through the Boston Public Library long ago. Politicians are not the only ones who don't reveal their true sources of their speeches, scientists deliberately don't cite the work of other competitors, especially when it comes to procuring grants and funds.

What I find even more worrisome is the current state of affairs in Physics. I remember quite well the statement made by Peter Freund (University of Chicago) in the last "p-adic numbers in Mathematics and Physics", conference held in Belgrade in 2005 (a more recent meeting was held in Moscow in October 2007). Freund said: "The reason students (in Physics) go to graduate school is to get a letter of recommendation..." I was deeply troubled by this statement. This means that in the world of Physics today it does not matter what you know but who you know. This is shocking. The world of Physics resembles the Vatican, stratified into hierarchical layers of money and "intellectual" power. Ed Witten is the Pope,

followed by the cardinals at Princeton, Harvard,... archbishops, bishops, priests, nuns... and finally the students (slaves).

The high priests at the top are supposed to generate the seminal ideas while those at the bottom receive only the crumbs and such that they are condemned to spend most of their careers doing boring complicated calculations with full knowledge that their names will end up in the dustbins of History and Herstory. I say "Herstory" because nowadays one has to be politically correct. This reminds me of being criticized at the end of my talk in Belgrade (by a scientist who had female Ph.D. students) for having referred to the expression "alpha males of physics" and not including the words "alpha females".

To describe the level of pettiness and the ongoing back-stabbing practices perpetrated by scientists, I recall the story by Ramon Guevara when he was at the Abdus Salam International Center for Theoretical Physics in Trieste, Italy. A senior Pakistani scientist (more of an administrator, I leave his name out on purpose since I will not honor this individual by naming him) approached Ramon Guevara and told him that he should not speak nor collaborate with Carlos Castro (myself) because this will jeopardize his physics career. Guevara told him that we were just good friends and that we had no intention of collaborating, since he did not have at the time sufficient knowledge to work with Castro. In the first place, a Castro-Guevara article would not have been taken very seriously by the physics establishment for it would have been perceived as a crank. Another disappointment I've experienced was the breakdown of initial job promises offered to me in some occasions. For instance, I was offered initially by E. Abdalla the possibility to spend some time at the Physics Department in the University of Sao Paulo, Brasil. Afterwards the offer vanished into thin air due to "budgetary restraints". Shortly after, a Brazilian scientist from another Institute (than E. Abdalla), whom I met in Austin, Texas, and later on in Sao Paulo in 1999, told me that the so-called "budgetary restraints" story was untrue. Moreover, he told me that they (Univ. of Sao Paulo) had plenty of funds to the extent that they did not even know how to spend their surplus of funds (Sao Paulo is a rich state). I realized then that I was being lied-to and that someone sabotaged my chances of being there. Other job offers have failed due to being blacklisted by Ginsparg who runs the the electronic-preprints arXiv.org at Cornell University.

To finalize I will depart from myself and focus on others. At the onset of the industrial revolution science was initially thought as the greatest promise to relief the human race from the daily miseries of life; that it was going to bring riches and happiness to us all. Nothing could be further from the truth: most of the human race lives in slavery and poverty, while the 500 richest persons in the world have more money than billions of people. All we have to do to see the failure of a brighter future for the human race after the industrial revolution is to read the daily newspapers and/or go back to the beginning of this article. Last century was Physics' century whereas this one might belong to Biology as it has been heralded and trumpeted all over the the news-media. Wonder drugs to reverse aging and cure all psychological and physical illnesses, organ replacements, genetic engineering programs (for the rich and select few only) to produce beautiful and intelligent super-humans, stem cells research, the creation of synthetic life in the laboratory and all sorts of artificial life forms, including intelligent ones, nanotechnology,... and so forth and so forth are part of the daily dosage of news paving the way for a magical world devoid of pain, anguish, suffering,... In the same

way that science did not deliver the promise of worldwide happiness after the industrial revolution, I am quite skeptical that Biological wonders will deliver happiness to us all because of the lack of ethics among many scientists, businessmen and politicians.

Politicians start wars, scientists build weapons and businessmen make astronomical profits with a total disregard of the consequences. The military-industrial-university-congressional complex that President Eisenhower warned us about in the 1950's is now a booming reality. Who knows how many trillions of tax dollars since 1950's have been spent. Some of us might have heard about the trillion-dollar amount that was unaccounted for in the Pentagon's expenses during the past decades. Pundits appear in our TV screens admitting daily that things are terribly wrong in the world but nobody seems to know how to solve these problems and prevent the impending catastrophes. The world is counting on us scientists to save the planet and/or provide them with the guidelines they must follow to attain a sustainable life on earth. Only if they knew what goes on in the corridors of power, universities, laboratories, corporations, weapons manufactories,...

All we can do is grasp to the cliché: hope for the best and be prepared for the worst. This is the way life is, some will say, and nothing can be done. If the genes are so unjust, how do we expect to have a just world to begin with? This is the question that has been haunting biologists for a long time. Whatever our most impending fears of the future might be, what is certain is that a worldwide scientific panel of ethics is desperately needed, where scientists may address all their grievances and they themselves can be brought to justice and be held accountable for their misdeeds. As far as oxymorons go, “ecological capitalism” is one of them. Let’s hope that “scientific ethics” is not another one.

“One thing is certain: Science has become the enemy of mankind. The instinct of omnipotency fills us with pride and leads to our self-destruction. To prove this one just needs to look at a recent poll: out of 700,000 highly qualified scientists working nowadays, 520,000 of them are vying to improve the means of delivering Death, to destroy humanity. Only 180,000 toil to find ways to protect us”

“From where will the treasures of goodness and intelligence spring in order to save us one day?”

[Quotes from the great Spanish film director Luis Buñuel, “Mi último suspiro” [My last whisper] (Plaza & Janés, Barcelona, 1982, pages 245-246)]